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A MANUAL OF LOGIC.

VOL. II.

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A

MANUAL OF LOGIC.

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PREFACE.

The present volume completes the work, the first volume of which was issued more than four years ago. In apology to my readers for this delay I can only plead the pressure of other work, and the desire not to place before them a hastily written treatise.

The doctrine of Induction here set forth is in substantial agreement with that developed by Dr. Bosanquet in his most philosophical book on *Logic*. To Dr. Bosanquet I owe a deep debt of gratitude for his kindness in giving me permission to make full use of his labours—a permission of which it will be seen I have freely availed myself. In this portion of my work I also owe much to the works of Mr. Bradley, Sigwart, Lotze, Jevons and Whewell.

In the treatment of Fallacies I would express my indebtedness to my friend, Professor Mackenzie, for his valuable help in the matter of classification. In this part

of my work I have ventured to quote largely from the chapter on Fallacies in De Morgan's *Formal Logic*, a book which has long been out of print and is not likely to be reissued. It seemed to me desirable that much that is acute and valuable in his treatment of this subject should be accessible to my readers. To the discussion of Fallacies in Dr. Davis's *Theory of Thought* and in Mill's *Logic* I am also indebted for help and suggestions.

In the book on Method it will be seen that I have generally followed the treatment of the Port Royalists.

J. W.

LEEDS,
January, 1896.

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BOOK V.

INDUCTION.

CHAPTER I.

POSTULATES OF INDUCTION.

141. Reality.

Induction is based upon the Principle of Sufficient Reason applied to Reality.

In Induction the material side of inference is predominant. It is the doctrine of those valid processes of thought by which knowledge of what holds universally in reality is attained. A consideration of those processes may, therefore, be conveniently prefaced by an enquiry into the presuppositions which are involved in the very idea of the possibility of such knowledge. We have as such a postulate the formal Principle of Sufficient Reason (*see* § 20[1]), and our task is now to show how our concept of reality harmonizes with, and gives content to, this formal postulate.

(1.) Empiricist View.

Empiricism reduces Reality to sensations,

It is the fundamental doctrine of the empiricist school of philosophy that all knowledge is derived from experience, and by 'experience' is meant mere succession of sensuous impressions. Thus, nothing can be in consciousness except sensations, and the reality of the universe reduces itself to a possibility of receiving such sensations. But a mere empty possibility is nothing, and, therefore, the empiricist position denies objective reality altogether ; the only reality

[1] First Edition, § 25.

Book V. Ch. I.

and this makes knowledge impossible.

for it is the series of sensations in an individual consciousness. And with objective reality goes, of course, objective or universal knowledge [*cf.* §§ 143 (ii.) (*a*); 145 (v.)]. A doctrine of induction cannot, therefore, be consistently based on empiricism.

(ii.) Thought and Reality.

Experience as thought is ultimate.

Yet it is undoubtedly true that we cannot get behind experience, that in experience we find our ultimate *datum;* in other words, that in experience alone we are brought into contact with reality. But when we say this, we are using 'experience' in a sense different from that of the empiricist. Experience is not simply what is *felt*, it is what is *thought*. Thus, experience is not a mere series of sensations but it is the totality of those impressions organized and systematized by the constitutive activity of mind. Phenomena are not mere transitory units in a series of ever perishing momentary sensuous impressions, but are elements of the real seen in relation to other elements. In brief, experience is concrete—*i.e.* real—thinking. There is thus no true opposition between thought and reality. There is not a world of minds on the one hand and a world of things on the other, requiring to be brought into relation with each other; but mind and object are in their essence, correlative. Mind conceived as a mere empty process of thinking is nothing; thought *is* thought only so far as it has content. And on the other hand existence out of all relation to some consciousness is also nothing; the real *is* real only as thought. As Green says: "The reality of things is their "determination by each other as constituents of one order, "a determination which only exists for thought. It is not "that there is first the reality of things, and then a theory "about it. The reality *is* a theory" (*Philosophical Works*, vol. ii., p. 269). The antithesis between fact and idea, theory, or law, is an aspect of that between thought and things, and is thus seen not to be absolute. "The true opposition is not "between thought and fact, but between thought and mere "feelings, which, except as related to each other through

There is no opposition between Thought and Reality.

"relation to thought, are not facts at all" (Green, *ibid.*, pp. 285-6). A "fact," *i.e.* an 'association of phenomena or appearances' (Herschel, *Nat. Phil.*, § 109), considered by itself is indeed no fact, for it has no nature; it exists only in various relations to other facts, and these relations only exist for thought, they are not open to sensuous observation. But the formulation of these relations is just what we mean by an 'idea' or 'theory' which embraces the fact [*cf.* § 145 (vi.)]. Now, every such relation is universal; *i.e.* it is applicable not to this one fact because of its individuality, but to this one fact as a type or example of a class of facts. Hence, whilst in sensuous impression we have facts presented in their particular aspect, in thought we grasp their universal aspect as examples of the operation of natural law; and experience, which includes both impression and thought, is thus seen to unite the universal and the particular. The antithesis between these therefore, is not absolute; it is but another aspect of the antithesis between thought and things. **All relation is universal.**

(iii.) Individual Experience.

Universal Experience includes all reality, but each individual has only his own particular experience, and, therefore, can only be said to come, in his experience, into contact with reality. "The real world," says Dr. Bosanquet, "is primarily "and emphatically *my* world. I take it to be real in virtue of "its contact with me" (*Logic*, vol. i., p. 82). And in individual experience reality is first known as that which constrains us to accept it as it is; that is real which, being presented to the senses, cannot but be perceived and that in a certain definite way determined *for* us, and not *by* us, which is what it is quite independently of our desires. And this involves that its character can be distinguished from, and compared with, that of other impressions. But such a reality is present *here* and *now*. We have then from the first the three categories which determine all experience as felt, *i.e.* as sensuous—the categories of quality, space, and time. And even in this momentary experience we have more than mere **Each individual comes into contact with reality in his own experience. All sensuous experience is determined in quality, time, and space.**

Book V. Ch. 1.

reception of sensuous impressions. For those impressions are synthesized by the mind into percepts of things, *i.e.* they are connected together in fixed and definite ways. Much of this we find ready done for us, and the results embodied in the common speech which we inherit. But experience is not merely momentary. The objects which are present to our perception now have been perceived more or less frequently in the past, and memory enables us to recognize them. Thus the categories get a fuller meaning. For our intellectual synthesis interprets these recurrent impressions in agreement with the law of Identity (*see* § 17[1]), and so we reach the idea of persistent things with persistent and determinate qualities. But there is not mere persistence, not mere formal and barren identity. Time involves change. Thus our idea of things includes both persistence and change, the change being that of the persisting substance. And this gives content to the mere formal identity; for real identity is only known amidst diversity. And, more important still, these persisting and yet changing things are not known in isolation; if they were we could not thoroughly systematize our experience. They are together in one space, and occupy various relations to each other. Change in an object implies change in its relations to other objects. We need, then, the idea of mutual interrelation and interaction to bring order into our experience. So the change in the persisting object is fully understood only when it is regarded as a change in its relation to other objects. And this brings us again to the universal, for it finds the very essence and meaning of things in their relations to other things, and all relations, as we have already said, are universal. Thus, the very nature of experience as known, and not as merely felt, involves that the world so far as known must be conceived as a systematic totality, with a thorough-going interrelation of parts. This conception is, doubtless, present with very varying degrees of clearness and fulness in different minds, but it cannot be altogether absent from even the rudest and most uncultivated mind, for without it experience itself would be meaningless, *i.e.* all knowledge would be impossible. Moreover, it follows

Continued experience is thought under the forms of persistence amidst changes, and interrelation.

Experience only becomes rational when the world is conceived as a system.

[1] First Edition, § 22.

that no individual's experience can be fully understood except as it is related to what is outside that experience, for thorough understanding of any part of reality is only possible when that part is known in its relations to the whole, and no one person's experience embraces the whole of reality.

142. Unity of Nature.

The Unity of Nature is the fundamental postulate of knowledge.

The above considerations have brought us to the fundamental idea which underlies our concept of reality, and therefore all our attempts to attain knowledge—the idea that nature is a unity. By this we do not, of course, mean that the universe is an unchanging identity, but that it is a system which remains identical with itself amidst the unceasing changes of relations between its parts, and which, by its own nature, necessitates and determines those changes. But every relation, and, consequently, every change of relation is a universal; in other words, it holds true everywhere and always, of all identical facts. Hence the idea of Unity of Nature implies that of Uniformity.

Unity implies Uniformity.

(i.) Origin of Principle.

Uniformity cannot be derived from observation,

Uniformity is thus seen to be a form without which intelligible experience would be impossible. It cannot, therefore, be derived from mere repeated sensuous experience, as Mill and other empiricist writers hold. Such a derivation would be possible only if observation gave us nothing but instances of uniformity. But as Mill himself says: "Every person's consciousness assures him that he "does not always expect uniformity in the course of events ". . . The course of nature, in truth, is not only uniform, it "is also infinitely various. Some phenomena are always "seen to recur in the very same combinations in which we "met with them at first; others seem altogether capricious; "while some, which we had been accustomed to regard as "bound down exclusively to a particular set of combina-"tions, we unexpectedly find detached from some of the "elements with which we had hitherto found them con-"joined, and united to others of quite a contrary description" (*Logic*, Bk. III., ch. iii., § 2). But as Nature is thus at once

both uniform and multiform in the sequences of phenomena which she presents to man's observation, it seems obvious that the idea that the multiformity is after all only apparent, and that if we examine deeper we shall after all find uniformity, cannot have been derived simply from observation in the sense of mere sensuous experience. Moreover, even were it otherwise, subjective expectation is not, and cannot give, objective knowledge, and thus can form no basis for inductive reasoning. It is only experience as thought, and not as merely felt, which is knowledge, and experience can only be thought under that very concept of uniformity which it is attempted to derive from it. The foundation of the error is that false antithesis between thought and reality to which we have already alluded. Mill tells us that, not only here but in all other cases of induction, we get the general idea by abstraction from the facts. But if by facts he means mere observation of the facts, *i.e.* experience as felt—and this meaning is most consistent with his general position—then it would be much more correct to say with Whewell, that we impose the conception on the facts. And if by facts we understand the elements of experience as thought, then it is plain that if we abstract the idea of uniformity from them, that idea must have been already present in them, for we cannot abstract what is not there. Again, this derivation of "the fundamental principle, or general axiom, of Induction" from "facts observed" (Mill, *ibid.*, § 1) renders the whole process of induction nugatory as a means of attaining knowledge, for it bases all rigid scientific inductions upon a simple generalization from unanalysed instances—an induction by simple enumeration, which Mill himself condemns as a means of attaining knowledge. Moreover, it is a very faulty example even of that process, for Mill himself defines such induction as "generalization of an observed fact from the mere absence "of any known instance to the contrary" (III., xxi., § 2), and he grants, as we have seen, that nature is far from exhibiting to observation any such uncontradicted unformity [*see* § 145 (v.)]. Nor is it easy to reconcile this empirical derivation of the principle of uniformity with the last

as it is involved in all understanding of experience.

This derivation of the principle of induction invalidates the whole process.

clause in Mill's statement that "The universe, so far as "known to us, is so constituted, that whatever is true in any "one case, is true in all cases of a certain description; the "only difficulty is to find what description" (III., iii., § 1). This, of course, is true. The chaos which nature seems to present on the first observation really vanishes upon deeper investigation. "But," as Green says, "it is just this "'interrogation' that has to be accounted for: it is only "upon the supposition of uniformity that we make the "interrogation. How can this be, if the supposition is only "derived from the observation of uniformity, an observation "which presupposes the interrogation?" (*Phil. Works*, vol. ii., p. 283).

The principle of Uniformity is not innate.

It is evident, then, that the principle of uniformity is not derived from experience in the sense of observation of facts. Nor is it an innate principle, born ready-made with every man. This Mill has no difficulty in establishing. He points out, what is indeed obvious, that: "As a general maxim . . . "it has scarcely entered into the minds of any but philosophers; "nor even by them . . . have its extent and limits been always "very justly conceived" (*ibid.*, Bk. III., ch. iii., § 1). But this fact is equally fatal to Mill's own position that the principle is derived from observation. As Sigwart says: "If we had "needed merely to open our eyes in order to see 'uniformity "in the course of Nature' everywhere before us, belief in the "thorough-going constancy of the way in which causes act "would not have been so slow to arise nor have been still only "a scientific and not a popular belief; nor would the tendency "to make capricious powers, demons and gods, responsible for "what happens in the universe have been so deeply rooted" (*Logic*, Eng. Trans., vol. ii., p. 111). It is, indeed, but an extension of that reduction of each individual's experience to a synthetic unity which we have seen to be implied in the simplest act of knowledge, and which is necessitated by the very fact that such experience is that of an individual subject. Thus, as Green says, "The conception on our part of nature "as a system, of which every part or process is determined "by relation to all the rest, is merely a development of this

It is a development of the reduction of individual experience to a unity.

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"original determination of our feelings by relation to one "thinking subject; and the reality of nature as a system "consists in the relation of its multiplicity to one thinking "subject, which distinguishes itself from it, but determines "it, makes it what it is, by this distinction of itself from it" (*ibid.*, p. 284).

(ii.). Meaning of Principle.

All uniformities must be thought as interrelated.

It has been proposed that, as nature exhibits to our observation multiformity as well as uniformity, it would be better to speak of the "uniformities"—rather than the "uniformity"—of Nature (*cf.* Mill: *Logic*, Bk. III., ch. iv., § 1; Minto: *Logic*, pp. 277-9). Such uniformities—or Laws of Nature—we undoubtedly seek to find; but to adopt a principle of uniformities, as the basis of our conception of nature, is to reduce the universe to a chaos. It is not in separate and independent sequences, that we can find that form of unity under which alone reality is thinkable; but only in a system within which they are all interrelated. The variety which appears to observation is just as much a part of the total system of the universe as are the instances of uniformity, and is equally necessary. Moreover 'uniformity' itself is not to be taken to mean 'resemblance.' It is in identity alone, not in mere resemblance, that we can find a firm basis of inference. By 'the uniformity of nature,' then, we do not mean, as Mill puts it, "that the unknown will be "similar to the known, that the future will resemble the "past" (*ibid.*, Bk. III., ch. iii., § 2). Such a general assumption that the future will resemble the past we have no right to make, and there is no necessity to make it. "The future," says Green, "might be exceedingly unlike the past (in the "ordinary sense of the words) without any violation of the "principle of inductive reasoning, rightly understood. If "the 'likeness' means that the experiences of sensitive "beings in the future will be like what they have been in "the past, there is reason to think otherwise. Present ex-"perience of this sort is very different from what it was in "the time of the ichthyosaurus. If it means that different

Uniformity does not mean mere resemblance, but identity of conditions.

"experiences of the future will be part of one system with "the present, the result of conditions that now are, it is "true; but to such a system and conditions the distinction "of past and future does not apply; they are eternal. On "the other hand, of that to which the distinction of past and "future does apply, resemblance cannot be truly predicated" (*op. cit.*, pp. 282-3). The other half of the assumption—that "the unknown will be similar to the known"—also gives us no basis for inference. From mere likeness of isolated phenomena we can draw no safe conclusions at any time; but only from identity of conditions. These considerations lead us to prefer to speak of the 'Unity' rather than the 'Uniformity' of Nature, as the postulate of induction.

(iii.) Scope of Principle.

A conception of the Unity of Nature is essential to understanding the world.

The importance of this fundamental principle to the organization of knowledge is well expressed by Mach. He says: "In the infinite variety of nature many ordinary events "occur; while others appear uncommon, perplexing, astonish-"ing, or even contradictory to the ordinary run of things. "As long as this is the case we do not possess a well-settled "and unitary conception of nature. Thence is imposed the "task of everywhere seeking out in the natural phenomena "those elements that are the same, and that amid all multi-"plicity are ever present. By this means, on the one hand, "the most economical and briefest description and communi-"cation are rendered possible; and on the other, when once a "person has acquired the skill of recognizing these permanent "elements throughout the greatest range and variety of "phenomena, of seeing them in the same, this ability leads "to a *comprehensive*, *compact*, *consistent*, and *facile conception* "*of the facts*. When once we have reached the point where "we are everywhere able to detect the *same* few simple "elements, combining in the ordinary manner, then they "appear to us as things that are familiar; we are no longer "surprised, there is nothing new or strange to us in the "phenomena, we feel at home with them, they no longer

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"perplex us, they are *explained*" (*Science of Mechanics*, Eng. Trans., pp. 5-6).

It is implied in all classification,

As the expression of the ground of all uniformity this general postulate lies at the root of all classification. The very idea of species under a genus involves that different classes of things partake in the same general nature, and that they are distinguished from each other by essential and regular differences, in other words that attributes persistently co-exist in a fixed and definite way. The same assumption is involved in the constitution of the species. Were it not for this constancy, knowledge would be impossible. If for example a substance could be identical with iron in every quality but one, the organization of experience could not even begin; for certainty would be unattainable. This constancy is implied in the category of substance and quality which we saw to be involved in the simplest act of knowledge [*see* § 141, (iii.)], and it is the postulate assumed in the formation of all generic judgments, which have been already said to be the only valid justification for the ordinary universal or general judgment of complete enumeration [*see* § 71, (i.), (*b*)].[1]

and in Causation.

An equally important aspect of this fundamental postulate is that of uniformity of mutually determined changes of relation amongst the elements of the universe. This leads us to the discussion of the principle of Causation.

143. Causation.

(i.) Function of the Concept of Causation.

Under Causation, must be considered both persistence and change.

(*a*) *General Statement of Question.* It has been seen above [*see* § 141, (iii.)] that our concept of Reality even in a simple form contains the antithesis between persistence and change, and that the idea of change can only be included in a unified system when it is regarded as change of relation between connected parts of that system. Thus, as Green says, "The "identical changes. Without identity there is no change, yet "change contradicts identity. To overcome the contradiction

[1] First Edition, *cf.* § 78 (ii.) *ad fin.*

"the change must be accounted for. At first the changes "seem chaotic. The first thing (*a*) to be done for satisfying "that demand for unity, which is the ground of our intelli-"gence and at the same time identical in principle with the "unity (the one subject) of the world, is to make out what "follows what; the next thing (*b*) is to account for the "ascertained uniformity by progressive discovery of its "conditions, a discovery which at once further ascertains the "uniformity and connects it with other uniformities of "change" (*Philosophical Works*, vol. ii., p. 302). We have here a brief but clear statement of the question we have now to investigate.

Causation is a postulate of knowledge, or a form of all experience.

(*b*) *Causation is a Postulate of Knowledge.* It must be noticed first that no sharp antithesis exists between the question as to the nature of causation, and that as to the origin of the idea of causation. Such an antithesis is but another instance of that false opposition between thought and things which has already been dealt with [see § 141, (ii.)]. For even if we examine the idea of causation from a purely subjective point of view, simply as it exists in our minds, we find it is but a reproduction more or less perfect and complete of the relation which actually exists. The statement that causation is a relation between phenomena can only be accepted if by 'phenomena' is meant not mere transitory and perishing impressions in an individual consciousness, but elements of real experience (*cf.* § 141). Causation is one of the forms in which alone we can think our experience, and it is therefore involved in all experience. That it is so is seen clearly in the very passage in which Locke states the empiricist position that the idea of causation is derived from observation. He says: "In the notice that our senses take "of the constant vicissitude of things, we cannot but observe "that several particular, both qualities and substances, begin "to exist; and that they receive this their existence from "the due application and operation of some other being. "From this observation we get our ideas of cause and effect. "That which produces any simple or complex idea we denote "by the general name, cause; and that which is produced,

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"effect" (*Of the Human Understanding*, Bk. II., ch. xxvi., § 1). Here it is perfectly clear that Locke is arguing in a circle, for it is only possible to observe "the application and opera-"tion" of things upon each other in the sense of thinking those changes under the form of causation. The idea whose origin Locke tries to account for by observation is, therefore, fully involved in those very observations from which he attempts to derive it.

Causation is grasped in varying degrees of fulness by different minds.

The fact that causation is thus a postulate of knowledge accounts for the variations in fulness with which the idea is grasped by different people. It is only the man of scientific thought who recognizes that every event is the outcome of uniform laws, though the complexity of their mode of action may be hidden from him. As Professor Huxley puts it: "Even thoughtful men usually receive with surprise the "suggestion, that the form of the curl of every wave that "breaks, wind-driven, on the sea-shore, and the direction of "every particle of foam that flies before the gale, are the "exact effects of definite causes; and, as such, must be "capable of being determined, deductively, from the laws of "motion and the properties of air and water" (*Hume*, p. 122). The conscious acceptance and application of the general axiom of causation—that every change must have a cause—is, therefore, a higher phase in the process of interpreting and unifying experience than that in which the existence and regularity of causation are recognized in particular instances.

(ii.) Nature of Causation.

Hume held that Causation is nothing but a feeling of expectation due to custom.

* (*a*) *Hume's Doctrine.* If we now ask what we are to understand by Causation we are met at once by the answer which, since the time of Hume, has been given by the empiricist school of philosophy—that causation is nothing but invariable sequence. With Locke causation implied power to produce change, yet the relation is not "contained "in the real existence of things, but something extraneous "and superinduced" (*op. cit.*, § 8), *i.e.* is an idea produced by the mind. Hume, seeing the untenability of this antithesis

between thought and reality, solved the difficulty by reducing all reality to felt experience—*i.e.* to series and combinations of sensuous impressions. Hence with him causation became entirely subjective. He says: "When any natural object or "event is presented, it is impossible for us, by any sagacity "or penetration, to discover, or even conjecture, without ex-"perience, what event will result from it, or to carry our "foresight beyond that object, which is immediately present "to the memory and senses. Even after one instance or "experiment, where we have observed a particular event to "follow upon another, we are not entitled to form a general "rule, or foretell what will happen in like cases. . . . But "when one particular species of event has always, in all "instances, been conjoined with another, we make no longer "any scruple of foretelling one, upon the appearance of the "other. . . . We then call the one object, cause; the "other, effect. . . . It appears, then, that this idea of a "necessary connexion among events, arises from a number "of similar instances, which occur, of the constant conjunc-"tion of these events; nor can that idea ever be suggested "by any one of these instances, surveyed in all possible "lights and positions. But there is nothing in a number of "instances, different from every single instance, which is "supposed to be exactly similar; except only, that after a "repetition of similar instances, the mind is carried by habit, "upon the appearance of one event, to expect its usual "attendant, and to believe that it will exist. . . . When we "say, therefore, that one object is connected with another, "we mean only, that they have acquired a connexion in our "thought. . . . We may, therefore, suitably to this experi-"ence, form [a] definition of Cause; and call it, 'an object "followed by another, and whose appearance always "conveys the thought of that other'" (*Inquiry concerning Human Understanding*, § 7). The language here hides the absolute subjectivity of Hume's view. But in his *Treatise of Human Understanding* he tells us: "What we "call a *mind* is nothing but a heap or collection of different "perceptions, united together by certain relations, and sup-

"posed, though falsely, to be endowed with a perfect sim-"plicity and identity. Now as every perception is distin-"guishable from another, and may be considered as separately "existent; it evidently follows that there is no absurdity in "separating any particular perception from the mind. . . . "If the name of *perception* renders not this separation from "a mind absurd and contradictory, the name of *object*, "standing for the very same thing, can never render their "conjunction impossible" (Part IV., § 2). And with Hume perception is nothing but feeling. "We may divide all the "perceptions of the mind into two classes or species, which "are distinguished by their different degrees of force and "vivacity. . . . By the term Impression . . . I mean all our "more lively perceptions, when we hear, or see, or feel, or "love, or hate, or desire, or will. And impressions are dis-"tinguished from ideas, which are the less lively perceptions, "of which we are conscious, when we reflect on any of those "sensations or movements above mentioned" (*Inquiry*, § 2). Hume's doctrine, then, has as its essential elements—(1) that reality is merely succession of sensuous impressions, (2) that causation is the idea of an invariable succession in our sensations, *i.e.*, is simply expectation, (3) that it is derived from repetition in experience of definite individual sequences.

Modern science rejects Hume's doctrine as to the subjectivity of Causation,

* (*b*) *Modifications of Hume's Doctrine.* The doctrine of causation adopted by many writers of the empiricist school in natural science is nominally that of Hume, but really not only differs from it, but is incompatible with it. Hume's conception of reality is necessarily rejected, for it is at variance with that very fundamental idea of the existence of a material world which alone makes natural science possible. They thus reject Hume's reduction of causation to mere belief or expectation. Causation is not with them, as with Hume, a "principle of connexion among ideas" (*Inquiry*, § 3), but one between events in a material world. One result of Hume's doctrine must be that as there are various degrees of strength in expectation, so there are degrees in causation. So plainly is this the case that Dr. Karl Pearson, who adopts the fundamental empiricist position, is constrained

to give to causation a meaning utterly opposed to that of Hume, in that he restricts it to the past, whilst Hume identifies it with an expectation of the future. Dr. Pearson says: "That a certain sequence has occurred and recurred "in the past is a matter of experience to which we give "expression in the concept *causation*; that it will continue "to recur in the future is a matter of belief to which we give "expression in the concept *probability*" (*Grammar of Science*, p. 136). Nor does the modern scientist grant that causation can only be proved by repeated observation. The chemist or physicist in working an experiment does not feel the need of repeating it an indefinite number of times in order to establish a case of causation; if he is sure that no unknown elements have crept in, he affirms causation on one single experience; and to do this, we have seen, Hume regarded as always unjustifiable. Indeed, often the element of observed sequence is disregarded and a hidden connexion is established by experiment, and in this inner connexion the causal bond is found and acknowledged, even though it differs from the observed elements of the sequence. But in such a case it is evident that causation cannot be derived from mere observed invariability of sequence. Indeed, the opposite is the case. A sequence is held to be invariable when all the conditions are so fully known that they are seen to be the sole and sufficient elements for the production of the change which is called the effect. When, for instance, hydrogen and oxygen are combined in the quantitative ratio of two to one and water is found to result from the combination, that connexion is immediately regarded as necessary, and, therefore, one of causation. In this case we have a hidden 'antecedent' which only manifests itself as an antecedent upon the performance of an experiment which produces the effect. It is this 'interrogation' of nature which makes the hidden causal bond manifest. But the idea of some causal bond must have been implicit in the mind of the original experimenter, or he would never have worked the experiment. Thus, experience is necessary to establish what causal connection holds in particular instances, but the idea of causal

and as to the establishment of the causal relation only by repeated observation.

connexion in general is not derived from those instances; it is essential to constitute them elements of an intelligible experience to all.

English writers generally regard succession in time as of the essence of causation.

Invariability of temporal sequence has, however, since the time of Hume held its ground in most English writers as the essence of the causal relation. Thus Brown in his *Essay on Causation* (p. 44) says: "A cause may be defined to be "the object or event which immediately precedes any change, "and which existing again in similar circumstances will be "always immediately followed by a similar change." This is obviously only a rough and ready statement of the vague and unanalysed notion of the plain man. Cause is here either an "object" or an "event." So in common speech either the object which acts upon another object, or the action itself is indifferently spoken of as the cause. A boy falls into the water, and either the water or the immersion is indifferently spoken of as the cause of the consequent wetness of his garments. A man takes arsenic, and either the substance or the act of taking it is indifferently named in common parlance as the cause of his death. But this is not scientific—*i.e.* exact—thought. The common idea needs to be analysed and made definite and precise.

Mill regards causation as invariable sequence between phenomena,

*(c) *Mill's Doctrine.* This was undertaken by J. S. Mill, the great exponent of induction from the empiricist standpoint. He begins by saying: "The notion of Cause being "the root of the whole theory of Induction, it is indis-"pensable that this idea should, at the very outset of our "enquiry, be, with the utmost practicable degree of pre-"cision, fixed and determined" (III., v., § 2). He then tells us that "the causes with which I concern myself are not "*efficient*, but *physical* causes. They are causes in that sense "alone, in which one physical fact is said to be the cause of "another. . . . The Law of Causation . . . is but the "familiar truth, that invariability of succession is found by "observation to obtain between every fact in nature and "some other fact which has preceded it. . . . To certain facts, "certain facts always do, and, as we believe, will continue to, "succeed. The invariable antecedent is termed the cause;

"the invariable consequent, the effect. And the universality "of the law of causation consists in this, that every consequent is connected in this manner with some particular "antecedent, or set of antecedents" (*ibid.*). "It is seldom, "if ever, between a consequent and a single antecedent, that "this invariable sequence subsists. It is usually between a "consequent and the sum of several antecedents; the concurrence of all of them being requisite to produce, that is, "to be certain of being followed by, the consequent. In such "cases it is very common to single out one only of the antecedents under the denomination of Cause, calling the others "merely Conditions. . . . The real Cause is the whole of "these antecedents; and we have, philosophically speaking, "no right to give the name of cause to one of them, exclusively of the others. . . . The cause, then, philosophically "speaking, is the sum total of the conditions, positive and "negative, taken together; the whole of the contingencies "of every description, which being realized, the consequent "invariably follows. The negative conditions . . . may all "be summed up under one head, namely, the absence of "preventing or counteracting causes" (*ibid.*, § 3).

but takes the cause to be the sum of essential antecedents,

In answer to the objection that if causation is nothing but invariable sequence day and night are the causes of each other, Mill says: "Invariable sequence . . . is "not synonymous with causation, unless the sequence, "besides being invariable, is unconditional. . . . We may "define, therefore, the cause of a phenomenon, to be the "antecedent, or the concurrence of antecedents, on which it "is invariably and *unconditionally* consequent" (*ibid.*, § 6). And by "unconditional" Mill explains that he means "what "writers mean when they say that the notion of cause "involves the idea of necessity," namely, "what *must* be "means that which will be whatever supposition we may make "in regard to all other things" (*ibid.*). As to the connexion of time sequence with causation Mill grants that "in some cases "(though those are a minority) the continuance of the con-"ditions which produced an effect is necessary to the con-"tinuance of the effect. . . . Whether the cause and its

and adds that the sequence must be unconditional.

"effect be necessarily successive or not, the beginning of a "phenomenon is what implies a cause, and causation is the "law of the succession of phenomena. . . . I have no objec- "tion to define a cause, the assemblage of phenomena, which "occurring, some other phenomenon invariably commences, "or has its origin" (*ibid.*, § 7).

These positions are not consistent with each other.

It has been thought well to give the substance of Mill's discussion of causation at some length, and in his own words, because of the authority his work so long enjoyed, and, though to a greatly diminished extent, still enjoys. But a careful examination of it will show that it has not really led us to that precise determination of the causal relation which was to be the result of the enquiry. Indeed, the position adopted at the end of the discussion is quite incompatible with that taken up at the beginning and which is nowhere consciously relinquished. Throughout, Mill holds to the idea of invariable sequence. Cause is the "invariable antecedent." By this Mill apparently means no more than when he says "Effect is the invariable consequent," for one of his main doctrines is that of the Plurality of Causes, which he thus expresses: "It is not true . . . that one "effect must be connected with only one cause, or assemblage "of conditions; that each phenomenon can be produced only "in one way. There are often several independent modes in "which the same phenomenon could have originated. One "fact may be the consequent in several invariable sequences; "it may follow with equal uniformity, any one of several "antecedents, or collections of antecedents. Many causes . . . "may produce death" (III., x., § 1). Thus, given a phenomenon we have no right, according to this doctrine, to speak of its "invariable antecedent" but only to say that it is the "invariable consequent" of some one of an indeterminate number of sets of antecedent conditions. But this doctrine of the Plurality of Causes not only conflicts with the definition of Cause as the 'invariable antecedent' but it is utterly inconsistent with the further conception of causation as 'unconditioned' sequence. If, as Mill tells us, given certain conditions we must necessarily, in the only intelligible sense

The doctrine of Plurality of Causes is inconsistent both with the definition of Cause as invariable antecedent,

and with the conception of unconditioned sequence.

of the word, find the effect—*i.e.* if the sequence is absolutely independent of all other things—it follows that each definite set of conditions can produce just one precise effect, and that no two different sets of conditions can produce precisely the same effect. Again, Mill sets out by discarding "efficient" causes, and yet he finds cause in a set of conditions whose existence necessitates that of the effect; greater efficiency than this no one would wish to establish. An efficient cause is one which is capable of producing a given effect, and according to Mill's own showing his antecedent conditions fulfil this requirement. It is the continual endeavour to retain time sequence at any cost which vitiates Mill's discussion, an endeavour due to his fundamental position that reality is nothing but phenomena—in the sense of mere transitory sensuous impressions—which are in their nature distinct and separate and are only conjoined for consciousness by the operation of psychological association.

Causation as unconditioned sequence is inconsistent with the rejection of 'efficient' causes.

(*d*) *Statement of Doctrine.* In speaking of cause as "sum of conditions" Mill really expressed, though somewhat awkwardly, the true doctrine. The expression "totality of conditions" would have been better, for totality suggests that the conditions are integral and interrelated elements in a system, whilst 'sum' rather suggests a mere numerical addition of independent units. It is in totality of conditions that we must find cause, and this is not dependent on time sequence. For if we analyse any case of causation we find that time sequence is not an essential aspect of it. The weight of the atmosphere is the cause of the height of the mercury in the barometer, but the two are coexistent. Contact with water wets, and contact with fire burns, but in each case the result is simultaneous with the contact. The fact to be accounted for, as was said at first, is change; but change implies something which changes. We cannot find the explanation of change in preceding change, for that would simply land us in an infinite regress. Nor can the explanation of all the changes an object undergoes be found in the nature of that object itself; the changes of the same object are too multiform and too evidently determined by

Cause is found in totality of conditions

and this is independent of time sequence.

Change involves persistency, and cannot be explained either as altogether immanent,

conditions external to the object to permit that. We cannot, therefore, regard each object as the *causa immanens* of its own changes. The common unphilosophical view is to regard all change as, on the contrary, due to the action of one body as agent upon another as passive recipient of action, and to ask what body is thus the *causa transiens* of such change. But further consideration shows that this also is insufficient. The same object produces different effects according to the nature of the object acted upon. Thus, as Bacon long ago pointed out, the sun hardens clay and softens wax. Similarly we know that "what is one man's meat is another man's poison." Hence we find that the attributes both of the agent and of the object acted upon are essential elements in determining the character of the change which is effected. Thus, persistence of attributes is essential to every instance of causation. Mill, indeed, acknowledges this, when he says: "In the enumeration of the conditions required for "the recurrence of any phenomenon, it always has to be "included that objects must be present, possessed of given "properties" (III., v. § 5).

or as altogether due to external influence, but necessitates the persistence of the attributes of both the agent and the object acted on.

This involves contiguity in space.

But if this persistence of objects and their qualities is necessary in every case of causation, and if the changes which an object undergoes cannot be explained by its own nature alone, but only when connected with changes in some other object, it follows that contiguity in space is also an essential element in causation; for it is only under the form of space that we can rationalize our experience of the influence of bodies upon each other. Thus change is seen to be at bottom a change in the spatial relations of objects which persist in time. And here it will be well to notice a difficulty which has been sometimes raised. It has been asked "How can a body act where it is not?" in other words how can we conceive a causal influence exerted on an object distant in space from the agent; as, *e.g.*, that of the sun on the planets. In reply to this it must be said that in one very true and important sense of its reality a body must be thought to be where its influence is felt; the power of exerting influence is one of its properties, and where, there-

Exertion of influence is a sign of the presence of a body in one aspect of its reality.

fore, that power is felt there the agent truly *is* in this, the only applicable, sense. Of course, in another sense of its reality—the sense in which reality is identified with visible and tangible form and tangible resistance—the body may be absent, but then that aspect of its reality is, in this case, beside the mark.

Time sequence is not a necessary aspect of causation.

The foregoing analysis has brought out that after all time sequence is not a necessary aspect of causation. Of course every change implies a prior state and a posterior state, but these do not stand in the relation of cause and effect. The dryness of a boy's clothes before his immersion in water is not the cause of their subsequent wetness; that cause can only be found in that spatial relation between the clothes and the water which we call contact. But if causation consists in certain spatial relations of bodies having persistent qualities, it is evident that whenever the cause is present the effect is present. The cause of the formation of water is the combination in definite proportions of hydrogen and oxygen, but this combination does not precede the formation of the water, it *is* that formation. And so it is in every other case. A weight in one scale of a balance holds suspended a weight in the other scale, but these are not successive in time. A dropping of ink upon paper causes a blot, but the blot is there as soon as the contact of ink and paper is made; it *is* that contact. Thus Hobbes had a much truer conception of cause than Hume and Mill and their followers when he defined it thus: "A cause simply, or an "entire cause, is the aggregate of all the accidents, both of the "agents how many soever they be and of the patients, put "together; which when they are all supposed to be present, "it cannot be understood but that the effect existeth with "them; or that it can possibly exist if any of them be "absent" (*Elem. Phil.*, II. ix. 121).

In popular thought time sequence is not in all cases regarded as an element in causation;

But, if causation is not essentially time sequence, it is necessary to show how the popular idea of sequence arose. This popular idea is not truly a general idea, that is to say, the plain man only applies it in cases in which it seems at first sight appropriate. He would grant, for instance, that con-

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tact with water wets at the moment of contact, and that the weight of the atmosphere and the height of the mercury balanced by it in the barometer are synchronous and not successive. But he would point to cases in which what he calls the effect is subsequent to that which he calls the cause.

and when it is, the selection of cause and effect is arbitrary.

Thus, *e.g.* he will say that a man takes poison first and that death follows at a longer or shorter interval. But here the words 'cause' and 'effect' are used very arbitrarily. By cause is meant the beginning of a chain of sequent events, and by effect one of those events selected because of its interesting character. But an infinite number of intermediate links can be inserted in the chain, each of which may be equally well regarded as the effect of that which precedes and the cause of that which follows, and thus the 'cause' is at best only the remote cause and is separated from its 'effect' by many intermediate proximate causes. What really should be said is that the persistent qualities of the poison being brought into spatial contact with those of the internal organs of the man, a changed condition of both the poison and those organs is brought about. A new set of persistencies is thus set up which is incompatible with the maintenance of life. But death is not the effect in any scientific sense of the word; it is only one element in the effect, which is in its totality those changed conditions which have just been mentioned. In this case we can easily make an arbitrary division, because in the series of changes which the continued reaction of poison and organs have upon each other there are two well-marked points—the taking of the poison and the death. And, according to the result, popular speech divides cause from effect at one or other or both of these points. If not death but only illness ensues, it would

The idea of sequence is only applicable when there is a well-marked point of time in the series of changes.

be said that the taking of the poison was the cause of the illness, the separation there being made at the earlier point of time; but if death ensues, the separation seems made at both points, and the interval between them disregarded. But we have not always such well-marked points of time when the persistent changes of the one body begin to show their influence on the other body, and in those cases the

sequence view of causation becomes very difficult of application. When we have such a well-marked moment then it is possible to draw an arbitrary line and say 'Here the cause ends and the effect begins,' but in that case it is evident that the end of the cause and the beginning of the effect are one and the same thing. Throughout, it is plain, the popular view treats cause and effect as two phenomena, *i.e.* events open to observation and separable from each other; it then regards that which comes first in time as the cause and that which follows in time as the effect. Moreover, in such a sequence of changes it is possible to stop at any point; that which has been up to that point is real in a sense which cannot be predicated of that which is yet to come, and so this actual reality is styled the cause of the potential reality which follows it. But immediately we analyse the sequence we see that the cause is not complete till the effect is secured, and we are thus led to regard cause, not as a phenomenal event in time but, as a totality of conditions whose existence secures the effect. Hence, the relation of causation is found in the securing of those conditions, which are, consequently, at once both cause and effect, and so temporal sequence ceases to appear as the essence of causation.

The continuance of the 'effect' after the cessation of the 'cause' depends upon the character of the persistent attributes.

The above considerations enable us to understand why in some cases the "effect" may endure an indefinite time after the "cause" has ended, whilst in other cases the old axiom—quite incompatible as it is with such continuance—*cessante causa, cessat effectus*, holds true. An example of the latter may be found in the withdrawal of the weight from one scale of an evenly poised balance, when the equilibrium of the two scales, which was the effect of the equality of the two weights, is at once destroyed. Examples of the former are found in the majority rather than in the minority (as Mill said) of cases of causation; for the changed conditions usually persist in a way open to observation. The most striking and instructive examples are found in mechanics. A blow sets a body in motion, and the motion not only continues after the blow is ended, but, according to the law of inertia, tends to endure for ever, and would actually do

so were it not counteracted by opposing 'causes,' such as gravity and friction. Again, if a moving body comes into contact with one which is immovable, the former is brought to a state of rest, and remains in that state so long as no other influence is exerted upon it. In this case we may speak of the fixed body as the 'cause' of the cessation of movement in the previously moving body, just as accurately as we may speak of the blow in the preceding example as the cause of the motion, or of poison as the cause of death. But in none of the cases are we using the word with scientific accuracy; it is the changed spatial relation combined with the permanent nature of each of the objects involved that is the true cause of the result, and that result is in each case nothing but the actual occurrence of that changed spatial relationship and the interaction of the permanent natures of the objects concerned which it involves. And this interaction is in the case of the examples quoted from mechanics that change in the visible attribute of motion of the bodies concerned which is expressed by the law of inertia.

Mechanical causation merges into the principle of the conservation of energy.

But these examples have led us to the further result that rest and motion are not fundamentally different in character, and that whether a body be at rest or in motion depends upon the equilibrium of forces within the total mechanical system of the universe. Thus the concept of causation is brought into relation with that of the conservation of energy.

In this connexion, too, we must notice that in the scientific analysis of any instance of causation, there is included the exact measurement of the amount of the change produced. The equivalence of cause and effect is here assumed, for the efficient amount of the cause is measured by the amount of the effect. When it is said in this connexion that "the cause equals the effect," the 'cause' spoken of is not a thing but the efficient action of the thing, and this action reduces itself to its permanent attributes in a certain spatial relation to the object on which it acts. Thus, for example, the amount of force exerted by a machine is measured by the amount of work it can do. These considerations need not be pursued further; they again lead us to the doctrine of the conservation of energy.

Result—cause and effect are identical in content but distinguishable in form.

We, then, arrive at this. Cause and effect are not two but one. That they are inseparable is indeed recognized by the relativity of the very terms themselves. A cause without an effect or an effect without a cause is a contradiction in terms and unthinkable. But we must go further and say that in *content* they are absolutely identical. It is only in *form* that they can be distinguished, and then we may speak of the one as determining, and of the other as determined. Thus the combination of hydrogen and oxygen in the quantitative ratio of two to one determines that the effect shall be water, and the character of that effect is determined by the character of the elements which are combined, but the combined elements and the water are one and the same identical substance, and this substance is the content both of the cause and of the effect.

(iii.) Axiom of Causation.

The selection of every instance of causation is arbitrary, both as to its beginning and end,

We have confined our consideration for purposes of simplicity to single instances of causation. But it must be noted that every such isolation, whether of a single change, of a group of changes, or of a series of changes, is an arbitrary act. Nature is continuous, and all existence is continuous and uninterrupted transition. Of course, in many cases, the boundaries of what we choose to call an instance of causation are plainly marked for observation, even as a strongly marked point has been seen to often form a convenient dividing line between cause and effect in the popular view of causation as temporal sequence. But the presence of such well-marked boundaries does not indicate any break in nature. Thus every cause is an effect and every effect a cause, and our attempts at explanation by the causal concept are involved in a regress which is practically endless as it only ceases in the conception of nature as a whole. But we have to notice a practical limit in another way—a limitation of the elements we take into consideration. Mill's whole system of the discovery of causes virtually resolves itself into an attempt to discover what elements in the total conditions under which a change takes place are essential and material to that

and as to its content.

change, and what can be disregarded. It is obvious that the totality of the circumstances attending any change can never be taken into account in our analysis of the conditions of that change, though only by doing so could we be said, in the fullest and most perfect sense of the word, to have explained that change. But this totality is utterly unwieldy; it is necessary to make a selection, and to find out what are the essential elements of that system of reality which is the ground for that change which we call in its various aspects cause and effect. It is to this necessity that Dr. Carus refers when he says: "The significance of cause and effect is to "a great extent arbitrary, and depends much upon the proper "tact of the observer" (*Fundamental Problems*, p. 84). Similarly, Mach says: "In speaking of cause and effect we "arbitrarily give relief to those elements to whose connexion "we have to attend in the reproduction of a fact in the "respect in which it is important to us. There is no cause "nor effect in nature; nature has but an individual exist-"ence; nature simply *is*. Recurrences of like cases in "which *A* is always connected with *B*, that is, like results "under like circumstances, that is again, the essence of the "connexion of cause and effect, exist but in the abstraction "which we perform for the purpose of mentally reproducing "the facts" (*op. cit.*, p. 483). In other words, in actual experiences the relations which we can observe are *impure*, that is the essential connexion we want to establish is more or or less overlaid with irrelevant concomitants. Höffding even goes so far as to say: "It may even be maintained that we "can never obtain absolute corroboration of the law of "causality by experience. The causal principle sets up an "ideal, which can never be fully realized in our cognition" (*Outlines of Psychology*, Eng. Trans., p. 212). Hence, there is the danger that the statement of the essential conditions may be imperfect either by inclusion of the accidental, by omission of the essential, or by both. It is because of this practical uncertainty that the axiom of causation—the same cause always produces the same effect—is frequently regarded as not reciprocal; that the simple converse—

In all observed instances of connexion, there are extraneous elements.

the same effect is always produced by the same cause—is not held to be justified. To this was due Mill's doctrine of the Plurality of Causes. But, as we saw, this doctrine is inconsistent with the view of cause as the totality of essential conditions. In any particular instance we may feel that we are not justified in regarding our statement of the causal relation as reciprocal, but that is because we are not certain that the whole of the essential conditions have been correctly sorted out. When we do feel that certainty, we do regard the statement of the causal relation as reciprocal. Thus, for instance, the statement that the combination of hydrogen with oxygen in certain definite proportions always produces water is held to state a reciprocal relation, *i.e.* to be simply convertible, and we feel equal confidence in asserting that pure water can be completely resolved into those two gases in those exact proportions. It is the same in all cases of scientific experiment where it is possible to make sure that all the essential conditions are known and are secured, and that no others are present which can modify the result. Thus, in every case where the establishment of a causal relation is certain, that relation is established as a reciprocal one.

Every instance of causation is reciprocal when the conditions are fully known;

Consequently the axiom of causation must be regarded as ideally reciprocal: our inability to apply it in its ideal form in any particular case cannot affect its character, but can only suggest to us a need for further enquiries till the true reciprocal condition is satisfied.

hence the axiom of causation is reciprocal.

The popular idea of the non-reciprocal character of the axiom of causation is due to the fact that the "cause" is much more frequently analysed than the "effect"—using those words in the popular sense of temporal antecedent and consequent phenomena. Thus, when Mill says in support of his doctrine of the Plurality of Causes, "Many causes may produce death" (*op. cit.*, Bk. III., ch. x., § 1), he is obviously speaking very loosely. Death, as we have pointed out, is not the whole effect. Moreover, death can never be death in general, but only some one particular kind of death, and the death caused by a bullet through the heart is not the same kind of death as that due to drowning, and both again differ from death by poison, and so on. The

The popular idea to the contrary is due to an insufficient analysis of the 'effect.'

effect as a totality differs in each case from that in every other case, and the very existence of the enquiries of coroners' inquests is a practical assertion of even popular belief in the reciprocity of the causal relation, as it assumes that by a careful analysis of the total 'effect' the cause is arrived at, and this assumption can be only justified on the ground that this totality could have had but one cause. Thus, the doctrine of Plurality of Causes will not hold, even with the view of causation as temporal sequence of phenomena, when a careful analysis is made of the so-called effect as well as of the so-called cause. Moreover such an analysis in exact proportion as it becomes thorough does away with temporal sequence, and brings 'cause' and 'effect' nearer and nearer together, till with the completion of the analysis they are seen to be identical in their content.

144. Ground or Reason.

(i.) Connexion of Ground and Cause.

We are now in a position to see how our conception of the world as a unity embracing changes determining each other under the form of causation gives content to the formal postulate of Induction—the Principle of Sufficient Reason (*see* § 20[1]).

Ground or *Reason* is the basis of a judgment.

Ground or **Reason** is that upon which a judgment is based as an act of thought. Now the true reason for a judgment is only to be found in the real system the inter-relation between whose parts the judgment expresses. A Ground is thus an identity including differences within itself, and it is perfect only when every essential element and relation in that system is accurately stated. In this case the hypothetical judgment which expresses the relation of the Ground to its consequent is reciprocal. But often in actual experience the Ground is not thus thoroughly known in all its essentials, and in these cases the hypothetical judgment cannot be simply reversed, and hence arises the formal rule which forbids the simple conversion of such judgments

When it is perfect the judgment is reversible.

[1] First Edition, § 25.

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(*see* § 105[1]). But an instance of causation in the final form to which our analysis in the last section led us is such a Ground; for there we have the identity of content as the system within which the difference of form between cause and effect finds its meaning. When the statement of the causal relation is perfect, *i.e.*, when all the essential conditions can be set forth—then it is an example of a perfectly defined Ground; when the causal relation is not thus thoroughly known, it corresponds to an incomplete Ground.

Every relation of causation is a Ground.

It is true there is a difference of aspect between Ground and Cause; the latter is the *causa essendi*—the cause why a change is what it is; the former is the *causa cognoscendi*—the cause of our knowledge of the event. And the *causa cognoscendi* need not be the *causa essendi*: it may be an effect or a mere accompaniment of the change. Thus, if I see the road to be wet it is a *causa cognoscendi* that there has been rain, though the rain was the *causa essendi* of the wetness. Or again if I look from the window of a train in which I am travelling and see the signal set against us, that is the *causa cognoscendi* to me that the train will stop, though the *causa essendi* of the stopping will be the application of the brake. But it will be noticed that in all these cases the *causa cognoscendi* is so only because we possess a knowledge of the real system within which both *causa essendi* and *causa cognoscendi* are related elements; it is not in any case a complete statement of the real ground of knowledge any more than it is a statement of the real ground in fact. On the other hand a *causa essendi* is always a *causa cognoscendi*, and hence the establishment of any causal relation is a real ground for the assertion of that relation.

***Causa essendi*= ground of existence; *Causa cognoscendi*= ground of knowledge.**

We may distinguish, then, between the relation of Ground and Consequence and that of Cause and Effect, but to oppose the two to each other is to again fall into that false antithesis between thought and reality to which we have already referred [*see* § 141 (ii.)]. The relations established in an instance of causation are the *Cause* of the change considered in regard to its reality, and its *Ground* considered in respect to its knowability. Again, in ordinary usage, Cause is not co-

Ground and Cause are distinguishable but not opposed.

[1] First Edition, § 119.

extensive with Ground but is confined to the explanation of events, whilst Ground refers to the foundation of all knowledge. "The distinction is, however, more one of usage than "of theory" (Bosanquet, *Logic*, vol. i., p. 265).

The ultimate Ground is the whole system of the universe,

Ultimately the only perfect and real Ground is the universe as a whole, for that only is unalterable, any change which takes place being merely a change within that whole and not an alteration of the whole itself as a whole. Similarly, we saw above that the universe as a whole is the ultimate limit of all possible causal explanation. But we cannot deal with the universe as a whole, and, therefore, we have to find within it, as starting-points of investigation, systems which are sufficiently self-contained for practical purposes, that is, within which we can find essential features which mutually determine each other in their changes. As these systems are understood their relations to each other within yet wider systems become evident, and so the process of systematizing the concept of the universe as a unity gets carried out with gradually increasing thoroughness and precision.

but smaller systems can be found within it,

though these are not separate from each other.

(ii.) Final Causes.

Final Cause =end or purpose of a thing's existence.

Lastly, the concept of Ground leads us to that of Final Cause, by which is meant the end or purpose for which a thing exists. For example, the final cause of a knife is the power of cutting, that of a watch the accurate measurement of time. It is here seen that the final cause is related to the efficient causes, by whose operation a thing comes to be what it is, as the end to which those efficient causes are the means. But the end gathers up the means into itself, and in it they find their only true realization. In the examples we have given the end is actually realized—the watch or the knife is made. But this actual realization is obviously not essential to the concept of final cause, for the final cause was the same even when the watch or knife was in process of manufacture. So that we may say with more exactness that a final cause is an end to be realized but which is not yet of necessity actual; for example, the final cause of study is the acquire-

ment of knowledge. And it is in this form only that we can apply the concept to the organic world. The final end of an organism is the notion or ideal of the nature of that organism, an ideal to which the doctrine of evolution leads us to believe the whole existence of that class of objects is a gradual approximation. In so far as the universe is a mechanism, its final cause is its stability as a mechanical system.

It is evident from even this brief treatment that in a true concept of the end we find a real Ground, and a Ground which embraces and yet goes beyond the idea of a mere mechanical system of the universe, and which thus satisfies the demand of the mind for rationality with the greatest attainable thoroughness.

CHAPTER II.

GENERAL NATURE OF INDUCTION.

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*145. Development of Doctrine of Induction.

The doctrine of Induction is modern.

There is not in Induction, as there is in Deduction, a large body of traditional doctrine which has been more or less generally accepted in its essentials since the time of Aristotle. On the contrary, the modern conception of the scope and importance of Induction, as the analysis of the processes of thought by which we systematize our knowledge of reality by thinking it under general laws, is of recent growth. It will be well, then, to approach our general consideration of the subject by briefly tracing the development of that body of doctrine, with special reference to English logicians.

(i.) Aristotle.

Aristotle regarded Induction as syllogism in Fig. III.

Aristotle treated the subject but briefly. Induction he described as ascending from the particular to the universal, in the sense of recognizing the universal in the particular. It is a kind of syllogistic argument in the Third Figure by means of which from two premises predicating respectively the major and middle terms of the minor, it is concluded that the major can be predicated of the middle, where "major," "middle" and "minor" refer to the varying width of the denotation of the three terms. Thus to take Aristotle's example—Let P be long-lived, M wanting bile, and S individual long-lived animals as man, horse, mule, etc. Then, the inductive argument is

$$S \text{ is } P = \text{Man, horse, mule, etc., are long-lived.}$$
$$S \text{ is } M = \text{Man, horse, mule, etc., are bile-less.}$$
$$\therefore M \text{ is } P = \text{Bileless animals are long-lived.}$$

This argument is demonstrative only when the predicate of

the minor premise is distributed. It has, therefore, an enumerative aspect, though this was not, with Aristotle, the most important one. Indeed as Sigwart points out (*Logic*, Eng. Trans., vol. ii., p. 292), Aristotle's "individuals" are not particular individual things, but species which he combines under a genus. Thus, not only does the aspect of rational connexion of content predominate, but the difficulty of obtaining a complete enumeration of "individuals"—which is insurmountable in the great majority of cases when those individuals are particular things—is not felt by him; he regarded an exhaustive summation of the species which compose a genus as quite feasible.

Aristotle regarded Induction as easier to be understood, though less demonstrative than Deduction, as it started from the particular, which is more easily cognizable as lying nearer to sense perception, and reached the universal, which though it is "absolutely prior and more cognizable" is yet more remote from sense perception.

'Proof by Example' is closely allied with Induction.

Closely allied with induction in Aristotle's doctrine, is the proof from the particular Example, which is in its essence analogical (*cf.* § 149). Thus, statesmen should not be chosen by lot because athletes are not so selected. The argument from example has, therefore, direct application to a particular instance, whilst the induction establishes the general proposition; but in each case the cogency depends upon a rational connexion of concepts.

(ii.) The Scholastic Logicians.

The *Scholastic Logicians* made induction entirely enumerative, and distinguished the process as *Perfect* or *Imperfect*.

The Scholastic Logicians neglected this rational element in their adaptation of Aristotle's doctrine, and made the essence of the induction to consist in enumeration. Thus arose the distinction between "*Perfect*" *Induction*—in which the subject is known to be a complete enumeration of the denotation of the predicate of the minor premise—and '*Imperfect*' *Induction*—in which this cannot be assured. This doctrine was practically universal amongst logicians till the time of Bacon, and has been upheld, even in our own day, by writers who regard logic as purely formal. Thus

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This is adopted by modern formal logicians.

Hamilton says induction is governed only by the formal axiom: "What belongs, or does not belong, to all the constituent parts, belongs, or does not belong, to the constituted whole" (*Logic*, vol. i., p. 321); and as this is simply the converse of the *dictum de omne et nullo* [*see* § 110 (ii.) (*a*)[1]], Induction is as purely formal as syllogism. He thus requires the minor premise in the "Inductive Syllogism" given above to be simply convertible, and he treats with contempt the objection that it generally is not so in fact. "This," he says, "is a very superficial objection. . . . For the logician "has a right to suppose any material impossibility, any "material falsity; he takes no account of what is objectively "impossible or false, and has a right to assume what pre-"mises he please, provided they do not involve a contradic-"tion in terms" (*ibid.*, p. 322). This is certainly pushing the antithesis between thought and reality to its utmost point, and does not leave it at all clear that logic—thus regarded—has any value whatever; it certainly is not an analysis of the processes of that real thought by which real knowledge is organized.

(iii.) Bacon.

Bacon is really a link between mediæval and modern philosophy.

Francis Bacon is often called the "Founder of the modern Inductive Philosophy" but he may with equal truth be spoken of as the last of the mediæval scholastics. "Bacon "frets himself to emerge from the mediæval doctrines, "amongst which he feels insecure, and again always falls back "into them" (Erdmann: *Hist. of Philosophy*, Eng. Trans., vol. i., p. 682). In one essential point at least he was behind his namesake, Roger Bacon, who more than three centuries earlier had advocated the use of experiment with a definite purpose, a method of interrogating nature which Francis Bacon practically forbids. He really forms a bridge between mediæval and modern speculation; whilst his doctrines were at bottom scholastic, yet he gave them a new empirical and utilitarian aspect. However, his advocacy of the study of nature had, from his exalted position and acknowledged talents, a powerful influence on men's minds.

[1] First Edition, § 124 (ii.) (*a*).

Bacon regards the acquirement of knowledge as an interpreting of nature; which is a gradual process. The mind must be cleared of *Phantoms* or false opinions—

Bacon regards the acquirement of all knowledge as a process of "interpreting nature," and his *Novum Organon* is intended as a "True Guide" to such interpretation. First, he condemns the scholastic method of discovering truth. This, which he calls "anticipating nature," he thus describes: It "leaps from the senses and particulars to the most general "axioms, and from these as first principles, and their un-"shaken truth, judges on and discovers medial axioms." In contrast with this his own method of "interpreting nature" is one which "raises axioms from the senses and particulars, "by ascending steadily, step by step, so that at last the more "general may be reached" (Bk. I., Aph. 19). Amongst the "anticipations" of which the mind must be cleared are the "phantoms" or "idola" or "false conceptions which have "hitherto preoccupied man's intellect, and are deeply rooted "in it" (Bk. I., Aph. 38), and which correspond in general to the fallacies of deductive logic. Of these phantoms Bacon enumerates four classes, of which the first two are intrinsic and the second two extrinsic:—(1) *Phantoms of the Tribe*, which dominate all men and "are founded on Human Nature "itself," so that men falsely interpret the impressions they receive through their senses (I., 41); (2) *Phantoms of the Cave* which are due to the peculiarities and limitations of each man's individual personality (I., 42); (3) *Phantoms of the Market-place* which arise "from the intercourse and "society of men with one another," *i.e.* errors due to language (I., 43); (4) *Phantoms of the Theatre* or false theories accepted because they are the fashion (I., 44). The mind being thus cleared of false prepossessions is ready to enter on the task of interpretating nature.

(1) Of the *Tribe*=common to man; (2) of the *Cave*=peculiar to the individual; (3) of the *Market-place*=due to language; (4) of the *Theatre*=due to fashion.

Broadly speaking, the fundamental features of Bacon's method are analysis and elimination, and it is thus, in its most general aspect, sound. But before it can be examined more in detail it is necessary to see upon what view of nature it is founded. It is briefly this:—Only individual bodies exist (II., 2) and each of these is a definite "troop or "combination of simple natures" (II., 5). Of these "simple natures," or abstract qualities, some belong to the "Form"

Bacon's view of nature is that bodies consist of 'simple natures,' of which th Form=the essence

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or "very essence" of the thing whilst others are derivative from these and are merely phenomenal (II., 13). The number of simple natures is definite and may be known (II., 19).

Bacon does not confine "Form" to one meaning. He speaks of "the Form of a given Nature" as "its true difference, or its causal nature, or fount of its emanation" (II., 1), "its very essence" (II., 13). Ultimately, it would seem that 'Form' should be found in the spatial and temporal relations of bodies [I., 75; II., 17 (3)], and so far as he adopts this view Bacon anticipates to some extent the doctrines of modern physics. In this doctrine of "natures" and "forms" there is much of the scholastic spirit as well as of its nomenclature; at the same time there is an attempt to give to the conception an aspect more in keeping with the position that knowledge of nature comes from observation.

Induction aims at discovering "Form,"

The object of Induction is to attain knowledge of causes, *i.e.* of "forms." "True knowledge is knowledge by "causes." In this Bacon agrees with all his predecessors. But his method is to be different: "For that Induction "which proceeds by simple enumeration is a puerile thing, "and concludes uncertainly, and is exposed to danger from "any contradictory instance, and for the most part pronounces from fewer instances than it ought, and of these "only from such as are at hand. But the Induction which "will be useful for the discovery and demonstrations of "Sciences and Arts, ought to separate Nature by due rejections and exclusions; and then, after a sufficient number "of negatives, to conclude upon affirmatives. . . . Now in "establishing Axioms by means of this Induction, examination and proof are also to be made use of; whether the "Axiom being established be only fitted and made to the "measure of those particulars out of which it is extracted, "or whether it is more extensive and broader" (I., 105, 106).

and proceeds by rejections and exclusions.

Three steps—(1) Collection of instances. (2) Sorting of instances.

There will then be three steps in the inductive process—(1) "a natural and experimental history must be prepared, "sufficient and good"; (2) the facts so obtained are to be arranged in Tables in an orderly manner so as to show (*a*) instances of the presence of the "simple nature" being

investigated, (*b*) instances of its absence, (*c*) instances in which it is present in varying degrees (II., 10-13). He recognizes, however, a difference of value in instances, and discusses fully under twenty-seven heads those which he regards as superior to others for inductive purposes and which he calls Prerogative Instances (II., 22-52). (3) Induction, whose "first duty . . . is the *rejection* or *exclusion* of "single Natures, which are not found in any instance in "which the given Nature is present; or which are found in "any instance from which the given Nature is absent; or "are found to increase in any instance, while the given "Nature decreases; or to decrease, while the given Nature "increases. Then after *rejection* and *exclusion* made in th "proper ways, there will remain . . . Form, affirmative, "solid, true, and well limited" (II., 16). If we examine this method we find that though Bacon says "The Syllogism is "not applied at all to the principles of Science" (I., 13), yet his method of inference is after all thoroughly syllogistic. "That upon which the whole process . . . is grounded is a "disjunctive major premise with many disjunctive members; "the minor premise is a copulative negative judgment, which "excludes all members of the disjunction but one; the "inference passes by the *modus tollendo ponens* to the "member which remains. The particular members of the "copulative minor premise which are combined by it are "derived by special syllogisms, which are all in the second "figure, in *Cesare* or *Camestres*" (Sigwart, *Logic*, Eng. Trans., vol. ii., p. 296).

(3) Induction is by exclusion and rejection.

The method is throughout syllogistic,

Thus, to take the example Bacon tried to work out to discover the 'Form' of heat:—

Let F = the Form of Heat; H = always present where heat is; A, B, C - - - X, Y, Z = various 'natures.' Then—

F is either A or B or C or - - - - X or Y or Z,

But *A is not H,*	And *B is* $\bar{H}$,
and *F is H,*	whilst *F is not* $\bar{H}$,
∴ *F is not A.*	∴ *F is not B.*

and so on with all the other exclusions till one only, say *X*, is left. The final syllogism then stands

F is either A or B or C or - - - - X or Y or Z,
But *F is neither A nor B nor C nor - - - - Y nor Z,*
∴ *F is X.*

It is, of course, assumed that the disjunction *A or B or - - - - Y or Z* is complete; and it is this very assumption that needs to be proved, and is most difficult to prove. Without it, the inference is obviously invalid.

is not that of science,

Thus again Bacon fails to escape from the traditional logic he disliked and contemned. At the same time his method is not that of modern scientific discovery. "The value of this "method," says Jevons, "may be estimated historically by the "fact that it has not been followed by any of the great "masters of science" (*Principles of Science*, p. 507). Throughout it is vitiated by the error of neglecting the work of mind in the constitution of knowledge, an error in which Bacon has been followed by all the empiricist school. "Men," he says, "should bid themselves for a while renounce conceptions, and "begin to make acquaintance with things themselves" (I., 36), ignoring the fact that "things" are nothing except in so far as they are understood, *i.e.* viewed by mind in their relations to other things; in other words, thought under concepts or made part of true experience. This ignoring of the work of intellect led him to speak of the acquirement of knowledge as a purely mechanical process. He says plainly: "Our method . . . of discovering the sciences is one which "leaves not much to acumen and strength of wit, but nearly "levels all wits and intellects" (I., 61).

and ignores the work of mind.

Bacon could not apply his own method.

Bacon himself failed absolutely in attempting to apply his own method. He was, indeed, not well acquainted with the natural science of his time, and had derived what knowledge of it he did possess entirely from books.

Bacon's aim is utilitarian.

The practical or utilitarian aim of Bacon is apparent throughout; man wants to interpret Nature in order that he may use his knowledge for his own advantage. Some of his remarks under this head show how far Bacon was from being really emancipated from mediæval ideas. One of his favourite examples is the transmutation of substances. Thus he says: "He who knows the Forms and methods of

"superinducing yellowness, weight, ductility, stability, melt-"ing, solution, and the rest, and their gradations and methods, "will see and take care that these may be joined together in "some body, whence may ensue a transformation into "gold" (II., 5). With Bacon there is, apparently, no idea of the pursuit of knowledge for its own sake; social utility seems to be with him the final aim of all human action.

(iv.) Newton.

Newton's method was (1) Analysis; (2) Formation of law; (3) Synthesis.

Newton was, of course, before all things a mathematician and physicist. But in his works he deals to some extent with scientific method. Thus he says in his *Optics* (qu. 31); "As "in Mathematics, so in Natural Philosophy, the investiga-"tion of difficult things by the method of analysis ought ever "to precede the method of composition. This analysis "consists in making experiments and observations, and in "drawing general conclusions from them by induction, and "admitting of no objections against the conclusions, but "such as are drawn from experiments or other certain "truths. . . . By the way of analysis we proceed from "compounds to ingredients, as from motions to the forces "producing them; and in general, from effects to their "causes, and from particular cases to more general ones, "till the argument ends in the most general." By synthesis, Newton means deductive reasoning from the general law which has been reached or suggested by the method of analysis, to determine whether the consequences of that assumed law or principle coincide with observed facts. There is thus between the steps of analysis and synthesis the formulation of some general law. Such laws are the expression of the effort of the mind to synthesize and explain the facts, and are what in modern logic and science are called hypotheses. Newton, however, used the term 'hypothesis' in the sense of those premature assumptions made on altogether insufficient grounds which were condemned by Bacon. Thus Newton says in the Scholium at the end of the *Principia*: "Whatever is not deduced from the phenomena, is to "be termed *hypothesis;* and hypotheses, whether meta-

Newton did not condemn hypotheses in the modern scientific sense,

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"physical or physical, or occult causes, or mechanical, have "no place in experimental philosophy. In this philosophy, "propositions are deduced from phenomena, and rendered "general by induction." It is evident from this, that his condemnation of hypotheses—strongly expressed in another place by the words "*hypotheses non fingo*" (I do not frame hypotheses)—is of something very different from what we here understand by the term, for the hypotheses we speak of always are "deduced from the phenomena." Indeed, Newton's own procedure throughout was by means of hypotheses, in the sense in which a modern scientist uses the term, and the four Rules for the conduct of enquiry into nature given at the beginning of the Third Book of the *Principia* are really rules for the formation and use of such hypotheses [*see* § 151 (i.)]. On the method and influence of Newton, Jevons says: "Newton's comprehension of logical "method was perfect; no hypothesis was entertained unless "it was definite in conditions, and admitted of unquestion- "able deductive reasoning; and the value of each hypothesis "was entirely decided by the comparison of its consequences "with facts. . . . It is a great mistake to say that modern "science is the result of the Baconian philosophy; it is the "Newtonian philosophy and the Newtonian method which "have led to all the great triumphs of physical science" (*Prin. of Sc.*, p. 583).

but used them continually, and gave rules to govern them

(v.) Mill.

Mill was an empiricist.

Mill adopted the fundamental empiricist position that nothing is given as a basis of knowledge but separate and particular sensations, which are originally merely subjective states of feeling. This position, taken up by Locke, was worked out by Hume with greater consistency than by any other writer. With him general truths—as we have seen in the instance of the law of causation [*see* § 143 (ii.) (*a*)]—are reduced to mere records of the "associations of ideas" in individual subjective experience, and statements of the greater or less strength of the expectation of the recurrence of similar chains of associations. This is a practical asser-

tion of the absolute impossibility of knowledge; for knowledge is, in its very essence, universal and objective, *i.e.*, independent of the individual consciousness. Mill shrank from this sceptical conclusion, and his doctrine of induction was intended to show from the empiricist standpoint how the attainment of knowledge is possible; *i.e.*, how it is possible to pass from the mere particular sensations given in experience, which he accepted as the only data, to what is not so given. Thus, he defines Induction as "the operation of "discovering and proving general propositions" (Bk. III., ch. i., § 2). Later on he expands this definition to: "Induc-"tion . . . is that operation of the mind, by which we infer that "what we know to be true in a particular case or cases, will "be true in all cases which resemble the former in certain "assignable respects. In other words, Induction is the "process by which we conclude that what is true of certain "individuals of a class is true of the whole class, or that "what is true at certain times will be true in similar circum-"stances at all times" (III., ii., § 1), and he tells us further that induction "consists in drawing inferences from known "cases to unknown" (III., ii., § 5).

The outcome of empiricism is that knowledge is impossible. Mill attempted to avoid this conclusion by his doctrine of Induction.

He defined Induction as 'discovering and proving general propositions.'

Now this process is obviously quite different from the "Perfect" Induction of the scholastic logicians [*see* (ii.)]. Of this Mill says: "This . . . is a totally different kind of "induction from ours; it is not an inference from facts "known to facts unknown, but a mere short-hand registra-"tion of facts known" (III., ii., § 1). This, therefore, is "improperly called" induction, for "any process in which "what seems the conclusion is no wider than the premises "from which it is drawn, does not fall within the meaning of "the term" (*ibid.*).

He denies that 'Perfect' Induction is truly induction.

Nor is it the same as the scholastic "Imperfect" Induction [*see* (ii.)], which assumed no such major premise. Of this Mill says: "The induction of the ancients has been well "described by Bacon, under the name of *Inductio per* "*enumerationem simplicem, ubi non reperitur instantia con-*"*tradictoria.* It consists in ascribing the character of general "truths to all propositions which are true in every instance

His doctrine differs from 'Imperfect Induction in requiring analysis of conditions.

"that we happen to know of. . . . In science it carries us "but a little way. We are forced to begin with it; we must "often rely on it provisionally, in the absence of means of "more searching investigation. But, for the accurate study "of nature, we require a surer and a more potent instru-"ment" (III., iii., § 2). This "more potent instrument" is "to be found in interrogating nature" by experiment, so as to find the conditions under which a phenomenon occurs.

Induction must transcend the known. This he justified by assuming the Uniformity of Nature as a major premise.

The essence, then, of induction is, with Mill, a transcending of the impressions given in experience. He finds the justification of this process in the assumption of the principle of the "uniformity of nature" (*cf.* § 142) which "will appear as "the ultimate major premise of all inductions" (III., iii., § 1). Every induction may, therefore, be expressed syllogistically thus—

> Under the same conditions the same event will always happen.
> Under conditions *x y z*, *E* has been found to happen.
> ∴ Under conditions *x y z*, *E* will always happen.

A single observation, therefore, will give us a valid induction, if the analysis of the conditions is accurate and complete. Mill grants this incidentally (III., iii., § 3 *ad fin.*), though the whole of his detailed treatment assumes that a plurality of instances is necessary.

This principle is obtained by induction by simple enumeration from other inductions of the same kind;

But, in accordance with Mill's general doctrine of inference [*see* § 138 (i.)[1]; § 143 (ii.) (*c*)] this general major premise—which he afterwards identifies with the law of causation—"There is . . ." he says, "no other uniformity in the events of nature "than that which arises from the law of causation" (III., xxi., § 4, note)—"is itself an instance of induction, and by no "means one of the earliest which any of us, or which man-"kind in general, can have made. We arrive at this "universal law, by generalization from many laws of in-"ferior generality. . . . As, however, all rigorous processes "of induction presuppose the general uniformity, our know-"ledge of the particular uniformities from which it was first "inferred was not, of course, derived from rigorous induc-

[1] First Edition, § 155 (i.).

"tion, but from the loose and uncertain mode of induction "*per enumerationem simplicem*" (III., xxi., § 2), and the induction by which the universal law is derived from these prior generalizations is of the same kind (*ibid.*, § 3). Hence, it is seen that this major premise, which Mill holds to be "certain," and "capable of imparting its certainty to all other "inductive propositions which can be deduced from it" (*ibid.*), is itself untrustworthy in a twofold degree; for it is an inference, uncertain in its very essence, from other inferences of the same dubious character. Mill attempts to avoid this conclusion by pointing out that "the precariousness of the "method of simple enumeration is in an inverse ratio to "the largeness of the generalization," and "the most exten-"sive in its subject-matter of all generalizations which "experience warrants, respecting the sequences and co-"existences of phenomena is the law of causation." "It is ". . . an empirical law co-extensive with all human experi-"ence, at which point the distinction between empirical "laws and laws of nature vanishes, and the proposition takes "its place among the most firmly established as well as "largest truths accessible to science" (III., xxi., § 3). This is true so long as we assume the uniformity of nature, but without that assumption it is altogether without meaning. Mill's argument on this point is, indeed, nothing but a *petitio principii*. We are, he says, "to consider no minor "generalization as proved except so far as the law of causa-"tion confirms it" (*ibid.*), and yet that law is to be derived from those very same minor generalizations which it is called upon to "confirm."

and is, therefore, doubly uncertain.

Mill says its certainty depends on its generality,

but this begs the question.

Further, Mill limits the universality of the law of causation. He tells us: "Empirical laws . . . can only be received "as true within the limits of time and place in which they "have been found true by observation" (III., xvi., § 4). Therefore, as the law of causation is an empirical law, it "must be received not as a law of the universe, but of that "portion of it only which is within the range of our means "of sure observation, with a reasonable degree of extension "to adjacent cases" (III., xxi., § 4). Mill does not see that

Mill limits the law of causation,

and this makes induction of universal propositions impossible.

this takes away all possibility of sure inference "from the "known to the unknown," or from one time to another. If we do not know the limits of time and space beyond which our inferences may not extend, it is obvious we can do nothing more than describe what has happened; we can never lay down any proposition as universally and necessarily true.

It is evident, then, that Mill's doctrine of induction breaks down. It is, at bottom, as destitute of certainty as is the scholastic induction which he condemns; for no process of inference can be of greater validity than the principle on which it is based; and it is, moreover, full of inconsistencies and contradictions.

Two incompatible theories of inference in Mill—(1) from resemblance; (2) from essential conditions.

Throughout Mill's *Logic*, indeed, we find two incompatible theories of inference—first, that inference is based on resemblance between phenomena, and second, that inference is grounded in the essential conditions of phenomena. The first which he calls "an universal type of the reasoning "process" is thus stated: "Certain individuals have a given "attribute; an individual or individuals resemble the former "in certain other attributes; therefore they resemble them "also in the given attribute" (II., iii., § 7). This is the foundation of his advocacy of the doctrine that "all inference "is from particulars to particulars" (*ibid.*, § 4), and is expressed in several of his definitions of induction. The second is found in his actual treatment of induction. He tells us that "to ascertain . . . what are the laws of causa-"tion which exist in nature; to determine the effect of every "cause, and the causes of all effects,—is the main business of "Induction" (III., vi., § 3), and that the process "is in some "sort a process of analysis," which is to be not only physical by experiment but mental; "the mental analysis, however, "must take place first." Moreover "the extent and minute-"ness of observation which may be requisite, and the degree "of decomposition to which it may be necessary to carry the "mental analysis, depend on the particular purpose in view" (III., vii., § 1). Much of the Third Book is taken up with an exposition of 'Methods' of carrying out such an analysis (*see* § 155).

In the former of these views Mill keeps fairly close to his empiricist position, for it is easy to confuse mere resemblance between phenomena with a succession of similar feelings in consciousness, especially when the word 'phenomena' is used ambiguously to denote sometimes elements of reality known in their relations—*i.e.* constituents of experience as thought; and sometimes mere sensuous impressions —*i.e.* parts of experience as merely felt. But the latter position is quite inconsistent with empiricism, for analysis of conditions—*i.e.* of relations—necessarily involves the synthetic activity of thought and is, therefore, rational. The essential relations, as we have already seen, are those of identity, time, and space [*see* § 141 (iii.), 143 (ii.) (*d*)], which are necessary in order that there may be objects to resemble each other. But our knowledge of these relations cannot be derived from the observation of resemblance, for resemblance in respect to them can only be known when the objects so resembling are already related in space and time.

The former view is empirical, but the latter rational.

The presence of these two unadjustable views accounts for many of the inconsistencies with which Mill's work abounds. He generally denies, in accordance with his fundamental empiricist position, the part played by mind in forming conceptions to explain facts as observed. Thus, he denies that Kepler's law of the elliptical orbit of Mars is an induction; it is, he says, only a description, and superadded nothing to the particular facts which it served to bind together; "the ellipse was in the facts before Kepler recog-"nized it. . . . Kepler did not *put* what he had conceived "into the facts, but *saw* it in them" (III., ii., § 4). Of course, Whewell had no difficulty in pointing out that "the "fact of the elliptical orbit was not the sum of the observa-"tions *merely;* it was the sum of the observations, *seen under* "*a new point of view*, which point of view Kepler's mind "supplied. Kepler found it in the facts, because it was "there, no doubt, for one reason; but also for another, "because he had, in his mind, those relations of thought "which enabled him to find it" (*Phil. of Discovery*, pp. 256-7; *cf.* (vi.) below). Mill, however, says "this is to confound two

These views account for Mill's inconsistencies.

He generally denies need of conceptions in induction,

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"very diffeient things, Invention and Proof," and "Induc-"tion is Proof" (*ibid.*, § 5), apparently forgetting that he had defined it as *discovery and* proof. He goes on, "The intro-"duction of a new conception belongs to Invention. . . . "But it is so far from constituting induction, that induction "does not necessarily stand in need of it" (*ibid.*), though he had told us in the preceding section that "without the pre-"vious colligation of detached observations by means of one "general conception, we could never have obtained any basis "for an induction, except in the case of phenomena of very "limited compass" (*ibid.*, § 4).

but cannot altogether dispense with them.

Thus, he cannot consistently exclude conceptions, and, as we have already seen he points out that the mental analysis of conditions is carried out with a "particular purpose in view."

Mill denies on empirical grounds the existence of Mathematical Inductions.

To Mill's empiricism is also due his denial of the name induction to the general propositions of mathematics, because "the truth obtained, though really general, is not believed "on the evidence of particular instances" (III., ii., § 2). It is, of course, really based on Mill's other ground of inference —the exact analysis of conditions. A good example of such Induction—which he calls "induction by connexion"—is given by De Morgan: "As an easy instance, observe the "proof that the *square* of any number is equal to the sum of "as many consecutive odd numbers, beginning with unity, as "there are units in that number: as seen in $6\times6=1+3+5$ "$+7+9+11$. Take any number, n; and write n n's (repre-"senting a unit by a dot) in rank and file. To enlarge this "figure into $(n+1)(n+1)$'s, we must place n more dots at "each of two adjacent sides, and one more at the corner. So "that the square of n is turned into the square of $n+1$ by "adding $2n+1$, which is the $(n+1)$th odd number. Thus "100×100 is turned into 101×101 by adding the 101st odd "number, or 201. If then the theorem alleged be true of "$n\times n$, it is therefore true of $(n+1)\times(n+1)$. But it is true "of the first number, 1×1 being 1; therefore it is true of "the second, or $2\times2=1+3$; therefore it is true of the third, "or $3\times3=1+3+5$; and so on" (*Formal Logic*, p. 212).

To the presence of the two incompatible theories of in-

ference is due the uncertainty of view into which Mill falls on another important point. In one place adopting empiricist ground he says: "I cannot concede that [the] recognition of the sufficiency of the evidence—that is, of the "correctness of the induction—is a part of the induction "itself" (II., iii., § 8); whilst later on, leaning to the rational position, he affirms that: "It is the number and "nature of the instances . . . that makes them sufficient "evidence to prove a general law" (III., ii., § 1), where, obviously, the recognition of the sufficiency of the evidence is regarded as essential to true inductive inference.

Mill contradicts himself as to the necessity of recognizing the *sufficiency* of evidence in induction.

One other inconsistency must be noticed. Mill first tells us, as we have seen, that the law of causation is "certain" (III., xxi., § 3). He then points out that in addition to uniformities of causation, which are all uniformities of sequence, there are also "uniformities of co-existence," some of which "cannot depend on causation" (III., xxii., §§ 1, 2). These statements of co-existence—"those which we have "reason to consider as ultimate, no less than those which "arise from the laws of causes yet undetected" (*ibid.*, § 7) are all merely empirical laws (*ibid.*, § 3), and depend upon induction by simple enumeration (*ibid.*, § 5), and therefore "are not to be presumed true except within the limits of "time, place, and circumstance, in which the observations "were made, or except in cases strictly adjacent" (*ibid.*, § 7). The origin of laws of uniformities of co-existence is, therefore, exactly the same as that of laws of causation, and it would be as easy to frame a general axiom in the one case as in the other. This, however, Mill denies: "There is no "general axiom, standing in the same relation to the "uniformities of coexistence as the law of causation does to "those of succession," and this deficiency "precludes the "application to the ultimate uniformities of coexistence, of a "system of rigorous scientific induction, such as the uni-"formities in the succession of phenomena have been found "to admit of" (*ibid.*, § 4). Hence, in the one case, according to Mill, a single application of simple enumeration can give no certainty, whilst in the other a double application—

Mill denies that statements of uniformity of co-existence are as certain as laws of causation, though they have the same origin.

i.e. a double uncertainty—gives certainty. The importance of the recognition of the necessity of uniformities of co-existence has been already pointed out [*see* § 143 (*d*)].

Mill's inconsistencies prove the helplessness of empiricism.

We may conclude these remarks on Mill's *Logic* in the words of Sigwart: "Taking away with one hand what he "gives with the other, Mill shows in the uncertainty of his "views the helplessness of pure empiricism, the impossibility "of erecting an edifice of universal propositions on the sand-"heap of shifting and isolated facts, or, more accurately, "sensations; the endeavour to extract any necessity from "a mere sum of facts must be fruitless" (*Logic*, Eng. Trans., vol. ii., p. 303).

(vi.) Whewell.

Whewell regards induction as the whole process of establishing general propositions.

The controversy between Mill and Whewell as to the nature of induction was partially due to different conceptions of its scope. With Mill "Logic is not the science of "Belief, but the science of Proof or Evidence" (Introd., § 4). In accordance with this, when arguing against Whewell he restricts induction to proof (*see* v.); though he had previously defined it as "the operation of *discovering and* proving "general propositions" (III., i., § 2), and though in his own treatment he adopts this wider view and discusses at length "The Four Experimental Methods" (*see* § 155) which he says "are the only possible modes of experimental enquiry" (III., viii., § 7). Whewell is more consistent, and treats induction throughout as the whole process of establishing general propositions. But his great point of disagreement with Mill was his insistence on the constitutive work of thought in the attainment of knowledge. Mill's empiricist position—that perception is a mere passive reception of particular sensuous impressions—we have already discussed (*see* v.). In opposition to this, Whewell truly asserts: "All "perception of external objects and occurrences involves an "active as well as a passive process of the mind;—includes "not only Sensations, but also Ideas by which Sensations "are bound together, and have a unity given to them. From "this it follows, that there is a difficulty in separating in our

As against Mill he insists on the work of thought in the attainment of knowledge.

"perceptions what we receive from without, and what we "ourselves contribute from within" (*Novum Organon Renovatum*, p. 51). It follows that "we cannot obtain a sure "basis of Facts, by rejecting all inferences and judgments of "our own, for such inferences and judgments form an un-"avoidable element in all Facts. We cannot exclude our "Ideas from our Perceptions, for our Perceptions involve "our Ideas" (*ibid.*, p. 53). We can, however, separate these elements by mental analysis. "We are not able, nor need we "endeavour, to exclude Ideas from our Facts; but we may "be able to discern, with perfect distinctness, the Ideas "which we include. We cannot observe any phenomena "without applying to them such Ideas as Space and Number, "Cause and Resemblance, and usually, several others; but "we may avoid applying these Ideas in a wavering or obscure "manner, and confounding Ideas with one another" (*ibid.*, p. 54). Hence, he well observes in another place: "At any ". . . step of Induction . . . the inductive proposition is a "*Theory* with regard to the Facts which it includes, while it "is to be looked upon as a *Fact* with respect to the higher "generalizations in which it is included. In any other sense ". . . the opposition of *Fact* and *Theory* is untenable, and "leads to endless perplexity and debate. Is it a Fact or a "Theory that the planet Mars revolves in an Ellipse about "the Sun? To Kepler, employed in endeavouring to com-"bine the separate observations by the Conception of an "Ellipse, it is a Theory; to Newton, engaged in inferring "the law of force from a knowledge of the elliptical motion, "it is a Fact. There are . . . no special attributes of "Theory and Fact which distinguish them from one another. "Facts are phenomena apprehended by the aid of conceptions "and mental acts, as Theories also are. We commonly call "our observations *Facts*, when we apply, without effort or "consciousness, conceptions perfectly familiar to us: while we "speak of Theories, when we have previously contemplated "the Facts and the connecting Conception separately, and have "made the connexion by a conscious mental act" (*ibid.*, p. 116).

Hence, he acknowledges no opposition between Fact and Theory.

In agreement with this fundamental position, "*Induction*

The essence of induction is the 'colligation of facts' by an appropriate 'Conception.'

"is a term applied to describe the *process* of a true Colligation of Facts by means of an exact and appropriate Conception. *An Induction* is also employed to denote the *proposition* which results from this process" (*ibid.*, p. 70). It follows that such a formula of Inductive Logic, as "'These particulars, and all known particulars of the same kind, are exactly included in the following general proposition,' . . . is not sufficient: for the particulars are not merely *included* in the general proposition. It is not enough that they appertain to it by enumeration . . . the Inductive step consists in the *suggestion* of a conception not before apparent. . . . Hence our Inductive Formula might be something like the following: 'These particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition'" (*ibid.*, pp. 110-1). Or better, as a perfectly carried-out Induction can give a conclusion of demonstrative certainty: "The several Facts are exactly expressed as one Fact, if, *and only if*, we adopt the Conception and the Assertion of the inductive inference" (*ibid.*, p. 113). The important point, then, is that "in every inference by Induction, there is some Conception *superinduced* upon the Facts: and we may henceforth conceive this to be the peculiar import of the term *Induction*" (*ibid.*, p. 74).

This is right if correctly interpreted,

Now, as Green well points out: "All science may rightly be described as progressive 'colligation of facts' through superinduction of conceptions if it be understood (*a*) that 'conception' means relation, which is rightly called 'conception' because it is constituted by the combining action of thought upon a manifold; (*b*) that every fact is constituted by such a superinduction; (*c*) that thus the colligating conception does not exist in our minds before or apart from its existence in fact; (*d*) that that on which it is superinduced is not the fact as it really is, but either (1) feelings on the part of us who feel before we understand, or (2) a fact as yet imperfectly conceived by us, not conceived in the fulness of its relations" (*Phil. Works*, vol. ii., pp. 288-9). But "Whewell . . . spoils his own case [against

"Mill] by often writing as if the antithesis between ideas "and facts were a valid one; as if the 'superinduction of "ideas' upon facts were merely an operation that had to be "performed *ex parte nostra* in order to give science" (*ibid.*, p. 285). Interpreted truly, Whewell's doctrine is practically the same as that which Mill adopts when he regards inference as the determination of the exact conditions of phenomena. Whewell's controversy, then, is with Mill's general empirical position, not with the incompatible rational theory which is also present in Mill's *Logic* [see (v.) above].

and agrees with Mill's inference by determining conditions.

Now the conceptions by which facts are to be 'colligated' are hypotheses, and these "may be useful, though involving "much that is superfluous, and even erroneous; for they "may supply the true bond of connexion of the facts; and "the superfluity and error may afterwards be pared away" (Whewell, *op. cit.*, p. 70). They should, however, be rigorously compared with facts and rejected if they do not stand this test (*ibid.*). One important aspect of such testing is that "our hypotheses ought to *foretell* phenomena which "have not yet been observed; at least all phenomena of the "same kind as those which the hypothesis was invented to "explain" (*ibid.*, p. 86). Especially important is the case when "the hypotheses which were assumed to account for "*one class* of facts are found to explain *another class* of a "different nature" (*ibid.*, p. 95). This Whewell calls "the "*Consilience of Inductions* from different and separate classes "of facts," and he points out that this tends to "*Simplifica-* "*tion of the Theory*. . . . For if these Inductions, collected "from one class of facts, supply an unexpected explanation "of a new class . . . there will be no need for new machinery "in the hypothesis to apply it to the newly-contemplated "facts. . . . The Consiliences of our Inductions give rise "to a constant Convergence of our Theory towards Sim- "plicity and Unity" (*ibid.*, pp. 95-6).

Whewell's 'conceptions' = hypotheses, which are confirmed by agreement with facts, and especially by a 'Consilience of Inductions.'

This being the nature of the inductive process, "the "*Logic of Induction* consists in stating the Facts and the "Inference in such a manner, that the evidence of the "Inference is manifest" (*ibid.*, p. 97), and, therefore, "The

The Logic of Induction is the criterion of Truth inferred from facts.

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"Logic of Induction is the *Criterion of Truth* inferred from "Facts" (*ibid.*, p. 98). This statement of facts and inferences should be set out in 'Inductive Tables' in which the "steps "of induction or of generalization" are arranged in succession, "so as to form a genealogical Table of each Induction, from "the lowest to the highest" (*ibid.*).

There are two stages of Inductive Inference—(1) Induction of Laws. (2) Induction of Causes.

In inductive inference there are two stages, which must not be confused. "Inductive truths are of two kinds, *Laws* "*of Phenomena*, and *Theories of Causes*. It is necessary to "begin in every science with the Laws of Phenomena; but "it is impossible that we should be satisfied to stop short of "a Theory of Causes" (*ibid.*, p. 118).

The former has three stages—(*a*) Selection of Idea. (*b*) Construction of Conception. (*c*) Determination of Magnitude.

The former process "may be considered as containing "three steps, which I shall term the *Selection of the Idea*, the "*Construction of the Conception*, and the *Determination of the* "*Magnitudes*. . . . By the word *Idea* (or Fundamental Idea), 'used in a peculiar sense, I mean certain wide and general "fields of intelligible relation, such as Space, Number, Cause, "Likeness; while by *Conception* I denote more special "modifications of these ideas, as a *circle*, a *square number*, a "*uniform force*, a *like form* of flower. Now in order to "establish any law by reference to facts, we must select the "*true Idea* and the *true Conception*" (*ibid.*, p. 187). Finally, if the resulting law is quantitative, the exact measurement must be determined. As to the first step, "no general "method of evolving such ideas can be given" (*ibid.*, p. 192). Their discovery is due to "happy guesses" which are then tested by comparison with the facts. The second step and the third are not altogether separable: "The Construction of "the Conception very often includes, in a great measure, the "Determination of the Magnitudes" (*ibid.*, p. 195). "In this "case also, the mode of arriving at truth is to try various "hypotheses;—to modify the hypotheses so as to approxi- "mate to the facts, and to multiply the facts so as to test "the hypotheses" (*ibid.*, p. 196). For exactly determining quantitative relations, Whewell gives four Methods—(1) of Curves, (2) of Means, (3) of Least Squares, (4) of Residues (*cf. ibid.*, pp. 202-219); whilst for determining qualitative

relations, "depending on resemblance" he gives the Methods of Gradation and of Natural Classification. Finally, the laws thus ascertained are to be verified by observation and experiment (*ibid.*, pp. 233-246). It will be noticed that Whewell makes the inductive process seem needlessly complex by the distinction he draws between Ideas, Conceptions, and Determination of Magnitudes. Put more simply his doctrine of method is practically the same as that of Newton [*see* (iv.) above], and resolves itself into the gradual determination by means of the analysis of facts of the exact form a hypothesis should take, and the subsequent verification of that hypothesis by a further appeal to facts. Whewell, however, emphasizes throughout the very element which Newton passes over with least notice, viz., the part played by the mind in constructing the hypothesis.

These must be followed by verification.

The final process is the induction of causes. Here Whewell tells us "we can no longer lay down any Special Methods by "which our procedure may be directed. . . . The principal "Maxim in such cases of Induction is the obvious one :— "that we must be careful to possess and to apply, with "perfect clearness and precision, the Fundamental Idea on "which the Induction depends" (*ibid.*, p. 248). We must, in other words, know the exact conditions which our hypothesis must satisfy, the exact scope of the empirical laws whose operation it has to explain. And we are to push our inductions as far as possible. "The first Induction of a "Cause does not close the business of scientific enquiry. "Behind proximate causes, there are ulterior causes, perhaps "a succession of such. . . . This enquiry after ulterior causes "is an inevitable result from the intellectual constitution of "man" (*ibid.*, p. 250).

There are no special methods for the induction of causes.

(vii.) Jevons.

Jevons' theory of induction is founded upon a thoroughly empirical view of the universe, and is, consequently, itself of the same empirical character. "Nature," he says, "is to us "like an infinite ballot-box, the contents of which are being "continually drawn, ball after ball, and exhibited to us.

***Jevons* starts from an empirical view of the universe.**

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"Science is but the careful observation of the succession in "which balls of various character present themselves" (*Principles of Science*, p. 150). In accordance with this view, induction is, with him, essentially enumerative, and only the result of a "perfect" induction—"when all the objects or "events which can possibly come under the class treated "have been examined"—is certain. "But in the majority "of cases it is impossible to collect together, or in any way "to investigate, the properties of all portions of a substance "or of all the individuals of a race. . . . In all such cases "induction is *imperfect*, and is affected by more or less un-"certainty" (*ibid.*, p. 146). On this point he is very emphatic. "There is no fact," he tells us, "which I shall more constantly "keep before the reader's mind in the following pages than "that the results of imperfect induction, however well "authenticated and verified, are never more than probable" (*ibid.*, p. 149). Keeping to the enumerative view of induction he bases the whole process on the mathematical theory of probability, the very essence of which is that it merely counts instances and absolutely ignores their character (*see* § 157). "I am convinced," he says, "that it is impossible to ex-"pound the methods of induction in a sound manner, with-"out resting them upon the theory of probability" (*ibid.*, p. 197). This is the weak point in Jevons' doctrine, and it is, unfortunately, a vital one from a philosophical point of view. Induction, as we have already urged (*see* § 142), must be based upon the conception of nature as a systematic unity. From this it follows that by the determination of relations in that system we can attain certain and universal knowledge. But on Jevons' theory true knowledge is impossible; we can only reach probability, and probability is expectation. In this respect, Jevons' doctrine is inferior to Whewell's.

Hence, induction is essentially enumerative, and the results of imperfect induction are only probable.

He bases imperfect induction on the Theory of Probability.

Jevons' theory of the *method* of induction, however, is in its main features the same as that of Whewell. He thus states it: "All inductive reasoning is but the inverse appli-"cation of deductive reasoning. Being in possession of "certain particular facts or events expressed in propositions, "we imagine some more general proposition expressing the

Jevons holds with Whewell that induction is inverse deduction,

"existence of a law or cause; and, deducing the particular "results of that supposed general proposition, we observe "whether they agree with the facts in question. Hypothesis "is thus always employed, consciously or unconsciously. The "sole condition to which we need conform in framing any "hypothesis is, that we both have and exercise the power of "inferring deductively from the hypothesis to the particular "results, which are to be compared with the known facts. "Thus there are but three steps in the process of induc-"tion :—

and involves throughout the use of hypothesis.

"(1) Framing some hypothesis as to the character of the "general law.

"(2) Deducing consequences from that law.

"(3) Observing whether the consequences agree with the "particular facts under consideration" (*ibid.*, pp. 265-6).

146. Doctrine of Induction.

(i.) Basis and Aim of Induction.

Inductive Inference starts with the particular.

Throughout the examination of the development of the doctrine of induction which has occupied us in the last section, it has been evident that there is a general agreement that induction is essentially an analysis of the process by which a universal judgment about reality can be established, and that this process starts with the particular. Indeed this is necessarily so. For it is only in experience that we come into contact with reality [*see* § 141 (iii.)], and experience is always of the concrete; *i.e.* of objects and events presented to us in varying degrees of isolation. In the apprehension of these concrete elements of experience, however, we have already the constitutive work of thought, organizing and systematizing sensuous impressions. The whole process of inference is but an extension of this synthetic activity of thought. But this process may be regarded in two ways. There is the empirical view which sees the essence of generalization in the enumeration of instances, and finds its ideal in the complete summation of a 'perfect' induction.

No universal knowledge can be derived from enumeration,

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This, as we have seen, was the doctrine of the scholastic logicians, and was adopted by Jevons. Nor did Mill ever break entirely free from it. He acknowledges, indeed, that a single experiment may yield a general truth (III., iii., § 3), but he continually speaks of, and treats, number of instances as an essential aspect of the data of induction. Even Whewell is, apparently, not altogether devoid of the same idea, for with him 'colligation of facts' is the starting point of scientific inference. The necessary outcome of this doctrine is scepticism, for it really denies the possibility of true knowledge at all. True knowledge is knowledge of the necessary and universal laws of reality. But even when a complete enumeration of instances is attainable the knowledge arrived at is only empirical; we can say that this has been and is, but not that it necessarily must be. Its fullest possible expression is the general proposition *Every S is P*, not the generic judgment *S is P*—which alone is the true justification of such a proposition—or the hypothetical judgment If *S is M, it is P;* and these latter alone express necessity (*see* § 82[1]). Moreover, in no cases of importance is a complete enumeration attainable, and the ideal then becomes an infinite series. In this case nothing more than probability even as to simple empirical matter of fact can be attained; we are not justified even in asserting *Every S is P*, but, strictly speaking, only *Every S may be P;* which is formally only equivalent to the particular proposition *Some S's are P.* Hence, from mere number of instances we can never reach universal truth.

for enumeration neglects relations.

Enumeration neglects the fundamental fact that nothing exists in isolation, that the very essence of the existence of each particular lies in the relations which connect it with other elements in the systematic unity of reality. Hence, every

The particular expresses the universal.

concrete particular is an expression of the universal, and it is only when we thus apprehend experience that knowledge becomes possible. The particular can be understood only when it is regarded as the necessary expression of universal law, and the possibility of apprehending such law is a presupposition of all induction (*cf.* §§ 141-4). The universal, then, does not derive its validity from the particular instances, but

[1] First Edition, § 83.

is recognized as present in them. This, as we saw above, was implied by Aristotle [*see* § 145 (i.)].

The data of experience are complex,

But no particular of experience is merely the expression of one law. All the data of experience are concrete, and as concrete are exceedingly complex, and every element in this complexity is the necessary expression of law. In other words, every universal law is, in its manifestation, bound up with irrelevant details which may be different in every case. The universal identity is thus only expressed amidst diversity.

and general law can only be reached by analysis,

Consequently, the process by which we reach the knowledge of the exact form of any law of relation is one of analysis, and as such offers abundant opportunities for error. The relations as we conceive them in thought may not agree with the relations as they exist, and then we sooner or later come to contradiction in our experience and the necessity for a revision of our mental construction. It is in this necessity for an analysis, which throughout implies abstraction and the consequent liability to error, that the advantage of a plurality of instances is found.

and to this process comparison is an aid.

For the form of any universal law is often more clearly and easily seen when comparison of instances which are known, or believed, to exemplify it enables the observer to disregard unessential elements in each concrete particular. It is here that number of instances has a place in induction. They assist us in analysing the conditions of the given, but it is from those conditions, and not from the instances as such, that the inference is made. If the conditions can be exactly ascertained in a single instance—as in the case of many chemical experiments—then no plurality of instances is needed, unless, indeed, there is some doubt as to the accuracy of the experiment, when, of course, its repetition may be necessary to remove that doubt. The only cases in which an inference is made from number of instances as such is when it is impossible—at any rate for the time—to ascertain the conditions of the phenomenon in question; and then the inference is not inductive, but belongs to the domain of mathematical probability.

Summary.

We see then that the aim of induction is knowledge of universal laws; that such laws are abstract, for concrete

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reality only presents us with them encumbered with extraneous detail; that, consequently we can reach them only by an analysis of the given, and in this analysis comparison of instances may be helpful but is not essential; that abstraction offers opportunity for error, and that, therefore, the form in which we think universal laws may be subject to modification, till they are conceived as consistently interrelated within a system. The expression of the laws which result from induction, therefore, advances from the generic judgment which expresses the necessary content of instances of concrete reality, to the abstract hypothetical judgment, or the disjunctive judgment, each of which expresses necessary universal relations within a system.

We may then define Induction in the words of Dr. Bosanquet: "Induction . . . in its most general sense consists in "satisfying the principle of sufficient reason by an analysis "of experience, directed to revealing the true coherence of "differences within universals" (*Logic*, vol. ii., p. 118).

(ii.) Method of Induction.

The Method of Induction is the formation and testing of Hypotheses.

From what has been said it is obvious that the method of induction consists throughout in the framing of hypotheses to explain the phenomena given in experience, and the verification of those hypotheses by constant appeal to facts. For, if the universal is not derived from, but is exemplified in, the particular; if it is in the facts, and by its very presence gives meaning to those facts; then it follows that every attempt to organize experience, *i.e.* to constitute reality for ourselves, is the formulation of a supposition as to what the universal law of relation is which is exhibited in the particular phenomena before us. And such a supposition is a hypothesis. Further, as the very nature of mind impels us to unify our experience, and as this can only be done by reaching a consistent view of the universe as a system, it results that advance in knowledge involves clearer apprehension of the exact nature of the universal relations within this system, and of their connexion with and dependence upon each other. And this means giving greater exactness to the form of our hypotheses, till by their exact agreement with known reality

their absolute truth is established. Induction is, therefore, an inverse process; it is the finding major premises when the conclusions are given. This is, it will be seen, substantially the view taken by Whewell and Jevons. It is in opposition to the doctrine of Bacon and his empiricist followers, but is accepted by Dr. Bosanquet, Sigwart and De Morgan. Jevons, indeed, was wrong in basing his theory upon enumeration of instances, instead of upon connexions of real content. As a result he made the inverse process much more indeterminate than, according to our view, it really is; for with him it depended upon the exclusion of alternative hypotheses, whose number was determined, not by analysis of reality but, by a formal calculation of mathematically possible combinations. And as a further result, as the ideal of enumeration is the unattainable infinite series, his conclusions could never attain certainty. In opposition to this, the view here advocated is that the possibility of every hypothesis must be determined by a careful analysis of content.

Induction is thus an inverse process.

This method is, in its essential features, what Mill calls the "Deductive" Method (*Logic*, III., xi.) to which, however, he gives too limited an application, regarding it as applicable only to the more complex phenomena where several causes combine to produce an effect. The laws of these separate causes should, he says, be first determined by induction, *i.e.* by an application of the experimental methods, then follows deductive reasoning to determine "what effect any given "combination of those causes will produce" (*ibid.*, § 2). Finally there is verification by comparison with specific experience. Mill grants, moreover, that "to the Deductive "method . . . the human mind is indebted for its most con-"spicuous triumphs in the investigation of nature" (*ibid.*, § 3). He, however, refuses to call this process 'Induction,' which name he restricts to the process of generalizing from experience, and even the recognition of its validity is essentially inconsistent with the doctrine advocated throughout the greater part of his book.

This Method is practically Mill's 'Deductive Method.'

The essence of the method, and its opposition to that of empiricism, is clearly put by De Morgan: "Modern dis-

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De Morgan's statement of inductive method.

"coveries have not been made by large collections of facts "with subsequent discussion, separation, and resulting de-"duction of a truth thus rendered perceptible. A few facts "have suggested an *hypothesis*, which means a *supposition* "proper to explain them. The necessary results of this "supposition are worked out, and then, and not till then, "other facts are examined to see if these ulterior results are "found in nature. The trial of the hypothesis is the *special* "*object;* prior to which hypothesis must have been started, "not by rule, but by that sagacity of which no description "can be given, precisely because the very owners of it do not "act under laws perceptible to themselves. The inventor of "hypotheses, if pressed to explain his method, must answer "as did Zerah Colburn, when asked for his mode of instan-"taneous calculation. When the poor boy had been bothered "for some time in this manner, he cried out in a huff, 'God "'put it into my head, and I can't put it into yours.' Wrong "hypotheses, rightly worked from, have produced more "useful results than unguided observation. But this is not "the Baconian plan. . . . What are large collections of facts "for? To make theories *from*, says Bacon; to try ready-"made theories *by*, says the history of discovery" (*Budget of Paradoxes*, pp. 55-6).

Steps of inductive method.

The essential steps in the inductive method then are:—

(1) The formation of a hypothesis suggested by a first observation of facts.

(2) The deduction of the consequences of this hypothesis.

(3) The testing of these consequences by a careful analysis of phenomena.

(4) The consequent exact definition of the hypothesis, which then, as expressing the true universal nature of reality, is verified and received as an established theory or law.

(ii.) Relation of Induction to Deduction.

It is evident from the above that there is no opposition between deductive and inductive reasoning. Such antithesis

Inductive inference is based on deductive principles.

is only possible when induction is regarded as founded on enumeration; and this, as we have seen, can give us no truly universal propositions, and, if it could, would render syllogistic reasoning fallacious (*see* § 139[1]). But on the view of inductive method which is here set forth it is evident that the inference is based on deductive principles throughout. It is a derivation of conclusions from hypothetical premises, "and nothing," as Dr. Bosanquet says, "can be more deductive than the connexion of a hypothesis with the consequences by which it is verified" (*Logic*, vol. ii., p. 119). Every method of induction, indeed, in so far as it consists in analysis of the given, includes deductive inferences. Thus we saw above [*see* § 145 (iii.)] that the inference involved in Bacon's method of exclusions is deductive, and the same is the case with Mill's methods of experimental enquiry (*see* § 155).

The distinction between induction and deduction is one of aspect.

But there is, nevertheless, a distinction of aspect between induction and deduction. In induction, reality presents itself in concrete and partially isolated instances, and the task of inference is to discern the universal which is more or less hidden in those instances. In deduction, on the other hand, reality presents itself in its universal aspect, and the task of inference is to trace the presence of that universal in the differing and complex instances of its manifestation. The distinction is, therefore, solely one of the order in which the two aspects of reality are presented to us; and this difference can have no effect upon either reality itself or our final conception of it, when we know it in both its aspects.

[1] First Edition, § 156.

CHAPTER III.

ORIGIN OF HYPOTHESES.

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147. Suggestion of Hypotheses.

(i.) Meaning of Hypothesis.

Definition of Hypothesis.

As "Hypothesis is a name that may be applied to any conception by which the mind establishes relations between data "of testimony, of perception, or of sense, so long as that conception is one among alternative possibilities, and is not referred to reality as a fact" (Bosanquet, *Logic*, vol. ii., p. 155), the need for a hypothesis may be forced upon us by any element in our experience. Indeed, we are continually forming hypotheses. Every supposition we make so as to account for any event whatever is a hypothesis. No doubt, it is customary to restrict the name to suppositions made in the scientific investigation of any subject; but these do not differ in original character from the hypotheses of common life. The difference comes in later; the hypotheses of everyday life answer their purpose in a more or less rough and imperfect form, but the hypotheses of scientific thought need to be determined as completely and accurately as possible, till at length they satisfy all the demands made upon them as explanations of actual phenomena, and are accepted as demonstrated and established theories. To such hypotheses our discussion will be confined, for logic deals only with scientific—*i.e.* with exact and accurate—thought. It must be remembered, however, that, as Clifford reminds us, "Scientific thought does not mean thought about scientific "subjects with long names. There are no scientific subjects. "The subject of science is the human universe; that is to "say, everything that is, or has been, or may be related to

Hypotheses are not confined to science.

In scientific thought hypotheses must be made exact.

"man" (*Essays*, p. 86). In scientific thought—no matter what the subject matter may be—every regularity or irregularity in the occurrence of events, for which no reason is apparent, every imperfectly understood or unexplained fact is a call for a hypothesis. And every hypothesis is a guide to further enquiry till the ultimate goal of explanation is reached; for, as Herschel says: "We must never forget that "it is principles, not phenomena,—the interpretation, not the "mere knowledge of facts,—which are the objects of enquiry "to the natural philosopher" (*Natural Philosophy*, § 10).

(ii.) Forms of Suggestion of Hypotheses.

Hypotheses are suggested by—

Hypotheses, then, may be suggested in various ways, but these may be reduced to three main classes:—

(1) Enumerative Induction.

(*a*) *By Enumerative Induction.* Every observed regularity of connexion between phenomena suggests a question as to whether it is universal. Thus, to take a simple example from mathematics—it is easily seen by simple inspection that $1+3=2^2$, $1+3+5=3^2$, and so on. This suggests the hypothesis that in every case the sum of the first n odd numbers will be equal to n^2. Such a hypothesis may be tested by additional examples, and as it is found to hold in a continually increasing number of cases, the probability of its holding universally is strengthened. But it can never be more than an empirical law—*i.e.*, a description of what relation actually does hold—until its necessity is established by a consideration of the essential properties of numbers, and this takes us beyond mere enumeration [*see* § 145 (v.)].

(2) Conversion of Propositions.

(*b*) *By Conversion of Propositions.* Whenever a universal connexion is established between phenomena, we are led to enquire whether it is reciprocal, and this, again, is an enquiry which can only be answered by such an analysis of content as will bring to light the exact grounds of the connexion. Thus, every conversion of an **A** proposition, suggests a hypothesis. Formally, of course, we can only convert *Every S is P* to *Some P's are S* [*see* § 102 (ii.) (*a*)[1]]. But, as *Some* is quite indefinite, and may, in fact, be all [*see* § 71 (ii.)[2]] we have the suggestion that *Every P may be S*. This will hold if the

[1] First Edition, § 1!5 (ii.) (*a*). [2] First Edition, § 78 (ii.).

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The aim of science is to establish reciprocal relations.

connexion of *P* with *S* is grounded in the very essential nature of *S*, and not in some nature common not only to *S* but to other classes of phenomena. As science aims at discovering this essential ground, it only reaches its goal when it establishes such reciprocal relations. For instance, the relation between gold and aqua regia is only made definite when it is established, not only that gold is dissolvable in that acid, but that it is similarly affected by no other substance. In contrast with this we may take the common proposition 'All men are mortal.' The simple conversion of this would be 'All mortal beings are men.' As experience teaches us this is not true, it is evident that the ground of mortality is not to be found in those attributes of humanity which distinguish men from other organic beings. The search for a reciprocal relation will be satisfied only when the essential ground of mortality is explicitly conjoined with it in the judgment, and this is the case when we state that 'All living organisms are 'subject to death,' the simple converse of which is also true.

Similarly, a hypothetical proposition does not formally admit of simple conversion, *i.e.*, the assertion of the consequent does not warrant the assertion of the antecedent —*If S is M, it is P* will only give us the suggestion *If P is M, it may be S* (*see* § 105[1]). Here then, is a new hypothesis pointing out a new line of investigation. If it is proved that the one essential ground of the connexion between *P* and *S* is *M*, then the reciprocal relation is established. For example, 'If a triangle is right-angled the middle point of the hypotenuse is equidistant from the three vertices' is equally true when simply converted, for the ground of the relation is found in the fact that right-angled triangles, and those only, are inscribable in a semicircle. But, in every case, the reciprocal relation can only be established by a new line of enquiry, it cannot be reached by mere formal inference. Hence, the simple conversion of affirmative universal propositions always suggests a new hypothesis, *i.e.*, points out the possibility of a new relation whose actuality must be solved by a new process of enquiry.

(3) Analogy.

(*c*) *By Analogy*. But analogy is the chief source from

[1] First Edition, § 119.

which new hypotheses are drawn. Indeed, it would not be too much to say that all hypotheses which offer an explanation—as distinct from a mere description—of phenomena, take their rise from analogy. A relation which is already familiar in one class of phenomena suggests the direction in which to look for the explanation of a new set of phenomena which bear some resemblance to the former. As Lotze says: "A successful hypothesis is always due to "the attention paid to analogies" (*Logic*, Eng. Trans., vol. ii., p. 95).

Accidental discovery of law is guided by analogy.

The origin of the hypothesis may sometimes have a largely accidental character. For example, the laws of the internal structure of crystals were suggested to Haüy by his observing that in a crystal accidentally broken the fracture showed regular geometrical faces. Similarly, "Malus chanced to look through a double refracting prism "at the light of the setting sun, reflected from the windows "of the Luxembourg Palace. In turning the prism round, "he was surprised to find that the ordinary image dis-"appeared at two opposite positions of the prism. He "remarked that the reflected light behaved like light which "had been polarized by passing through another prism. "He was induced to test the character of light reflected "under other circumstances, and it was eventually proved "that polarization is invariably connected with reflection" (Jevons, *Prin. of Sc.*, p. 530). This latter case shows very clearly that, though accident had a share in the discovery of the connexion of polarization with reflection, yet it was an accident that would only have occurred to a student of optics. And so it is generally in such cases. The accidental occurrence would be unnoticed by a mind not stored with knowledge of cognate classes of phenomena, but to such a mind it is rich in suggestion, because numerous analogies are ready at hand to offer a possible explanation.

The problem to be solved may be a residual part of a phenomenon.

In other cases the problem requiring solution may be due to some unexplained residual part of a phenomenon. Thus, the deviations of Uranus from its predicted orbit showed that the whole phenomenon was not accounted for. Analogy

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with the deviations in the orbits of other known planets caused by their influence on each other led to the hypothesis that a planet as yet undiscovered existed, whose orbit was further removed from the sun than was that of Uranus; a hypothesis which was proved correct by the discovery, in 1846, of Neptune.

A plurality of hypotheses may be suggested.

Sometimes various analogies may suggest a plurality of hypotheses, which must be successively tested till all but one are, by disagreement with the facts, rejected. These alternative hypotheses may be simultaneously or successively suggested, but only one is taken up at a time. Thus Kepler records that he advanced nineteen hypotheses which he afterwards disproved, before he arrived at the true statement of the laws of planetary motion. He was misled by false analogies; "his favourite and long studied theory," says Jevons, "was founded on a fanciful analogy between "the planetary orbits and the regular solids" (*Pr. of Sc.*, p. 578). Had the study of Algebra been in a more advanced state at Kepler's time than it actually was, it is probable that he would much sooner have hit upon his third law—that the squares of the periodic times of the several planets are proportional to the cubes of their mean distances from the sun—for it is in algebra that the true analogy is found. But, as Whewell says, in a passage which shows clearly how analogy suggests hypotheses: "The process of connecting "two classes of quantities by comparing their *powers* is "obvious only to those who are already familiar with general "algebraic views" (*Hist. of Ind. Sciences*, vol. i., p. 323).

The logical character of the second of these three modes of suggestion of hypotheses has already been considered [*see* §§ 102 (ii.); 105[1]]. We will now discuss that of Enumerative Induction and of Analogy.

148. Enumerative Induction.[2]

(i.) Logical Character.

Inductive Inference does not start from perfectly unorganized experience. The impressions received through

[1] First Edition, §§ 115 (ii.); 119.

[2] The treatment of the next two sections is based on that of Dr. Bosanquet (*Logic*, Bk. ii., ch. ii. and iii.).

the senses are already synthesized into perceptions of objects and events, and these are classified on the basis of common attributes found amongst individual differences and peculiarities. Moreover, the classification is embodied in language, so that in the natural acquirement of language is involved a tacit reception of much of the first stage in the systematization of knowledge. It is the denoting different individuals by a common name that forces upon us the first recognition of a common nature, and leads to the beginning of conscious inference. Particular individuals, *a, b, c, d,* thus classed together by language under the common name *S,* are observed to possess a common attribute *P,* not known to belong to *S,* as such, and the question is suggested whether every other member of the class *S* also possesses this same attribute. Now this extension can only be logically justified by the conviction that *P* is grounded in the common nature of *S.* This common nature must, therefore, be a universal identity which finds expression in the concrete particulars, *a, b, c,* etc., though these differ from each other in many respects. But all particulars may be classed in various ways. Hence arises the first difficulty of induction—that of finding the appropriate common concept, under which to think the observed particulars ; and in the assumption of this we are obviously making what may be called a preliminary hypothesis, viz., that in the content of the selected *S* we find the ground of *P.* We should, *e.g.*, be obviously unjustified in arguing that because the pictures *a, b, c, d* are admirable works of art, therefore, any other picture *e* is also an admirable work of art ; for the ground of excellence cannot be thought as inherent in them simply as pictures. But if *a, b, c, d, e* are all works of the same great master, then our inference to the excellence of *e* from that of the observed *a, b, c, d,* is much more probable, as it is based on the identity of origin. The inference is not, however, certain, as it is not in this origin alone that the ground of excellence can be found.

Induction assumes the classifications embodied in language,

and starts from them

The first difficulty is to refer observed particulars to an appropriate class.

Now the proposition *Every S is P* is, in form, though not in meaning, merely a summary of individual judgments of perception which affirm *P* of every member of the class *S,*

The universal judgment is enumerative in form.

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though we are, in incomplete enumeration, attempting to base it upon only a limited number of these possible individual judgments. There are, thus, two elements in our premises—the assertion that *P* belongs to the observed particulars *a, b, c, d,* and the statement that *a, b, c, d,* are individual expressions of the universal identity of content *S*, which is believed to contain the ground of *P*. As *a, b, c, d,* are concrete instances, they are naturally made the subject of each of these propositions (*see* §§ 68 ; 84 [1]), and so the argument falls into the third syllogistic figure (*see* § 113 [2]) and may be thus symbolically expressed—

Enumerative induction is a syllogism in Fig. III.

a, b, c, d are P
a, b, c, d are S

∴ *Every S may be P*

In other words, the conclusion suggests *Every S is P* as a hypothesis worthy of examination, and our task is to attempt to establish this as a universal law, by showing that there is a sufficient ground for the connexion.

(ii.) Twofold Tendency of Enumerative Induction.

Two tendencies here become operative. There is, first, that which confines itself to the formal aspect of the proposition to be proved, and endeavours to establish the universal by a mere examination of the denotation of *S*. And there is, secondly, that which fixes attention upon the meaning of the proposition, and aims at establishing the generic judgment *S is P*, which is the only real justification for the denotative proposition *Every S is P* [*see* § 71 (i.) (*b*)[3]].

(*a*) *Tendency to Complete Enumeration.* Following out the first tendency it is evident that to deduce the universal conclusion from the given premises is formally unallowable, as it would involve the fallacy of illicit process [*see* § 111 (ii.)[4]). Thus, the incomplete enumeration suggests at first the attempt to reach the concrete universal of allness by making the enumeration of instances complete. But this is impossible in most cases ; it would lead us to an infinite series which

To draw a universal conclusion involves Illicit Process ; hence the ideal of complete enumeration.

[1] First Edition, §§ 75 and 93.
[2] First Edition, § 127.
[3] First Edition, § 78.
[4] First Edition, § 125 (ii.).

cannot really be summed. Moreover, mere enumeration of instances largely neglects the true bond of union, which is the common nature shared by all members of the class, and in which must be sought the ground of *P*. It reduces this common nature to mere agreement in denomination, whilst it disregards the characteristic peculiarities of the instances, which are all necessary and in relation to the universal nature, and degrades them into mere separability into units. Hence, the universal, if it could be reached by simple counting would be abstract, and concerned only with the numerical aspect of the concrete instances.

But in practice we do not confine ourselves strictly to the enumerative side of the process, *i.e.* we look for something more than mere *number* of instances. Indeed, mere number cannot take us very far. There must be some *specification* of the instances as well as numbering of them. Repetition of the same instance of connexion between *S* and *P* gives but little ground for the assumption that that relation is universal. But, if the instances examined differ from each other very largely, and have little in common except their agreement in exhibiting both *S* and *P*, then the probability that *S* contains the ground of *P*, is very largely increased. For example, the fact that all polar bears examined have white fur would give us little ground for the supposition that all animals which dwell among snow have white coverings. But when we find animals differing much in other respects—as the polar bear, the American polar hare, the snowy owl, and the Greenland falcon—agree in habitat and colour, the possibility of a regular connexion between the two is suggested. When, moreover, instances which at first seem to be exceptions are, on further examination, found to confirm the rule, its claim to generality becomes stronger. When the Arctic fox, the Arctic hare, the ermine and the ptarmigan, which are not always white, are found to become so when amongst the snow in winter, the truth of the suggestion that there is a universal connexion between white surroundings and white covering becomes much more probable. But even thus modified, enumeration can never get beyond a high

probability that *Every S is P*, for it can render no reason why that connexion exists, and the universal proposition is always liable to be overthrown by a single instance to the contrary. In other words, it can only describe what happens by an empirical law of greater or less probability; it can never reach explanation. Hence, the enumerative ideal, even were it attainable, is, from its very nature, incapable of giving that true concrete universal, or generic judgment, which is the assertion of necessary connexion of which we are in search.

The connexion of *P* with *S* must be established by analysis of the content of *S*.

(*b*) *Tendency to Analysis of Content.* In the second place, then, it is evident that our premises give us no exact conditions in which we can find the ground for a necessary connexion between *S* and *P*. We have seen that such an assumption is involved in the universal proposition we are attempting to establish, *i.e.* in the subsumption of our particular observed instances under *S*. And the assumption is based on a necessity of thought. For, by the principle of Identity (*see* § 17[1]) we are constrained to think every characteristic phenomenon as dependent upon one, and only one, definite condition or set of conditions. Now, if *P* belongs to *a, b, c, d,* it cannot be grounded in their individual differences, but only in their common nature *S*. The content of *S* must, therefore, be analysed, and if, as a result of this analysis, it is found to contain the ground of *P,* then the necessary connexion of *P* with *S* is established, and we reach the generic judgment *S is P,* which is the true concrete universal, and which, as not liable to be overthrown by contrary instances, is the real justification for the formally enumerative proposition *Every S is P*.

But, as *P* is not the only characteristic connected with *S,* it is evident that the content of *S* as a whole is not its simple ground—*i.e. S* is not the one definite condition of which we are in search. By analysis alone can be found what it is in *S* which necessitates *P,* and is thus the foundation of the universal connexion expressed by the generic judgment *S is P*.

There are, then, in this earliest form of inductive inference,

[1] First Edition, § 22.

two divergent tendencies; one which when carried further leads to arithmetical computation, and the other, whose general course has just been sketched, leading on through analogy—which is the first stage in the determination of content—to a thorough analysis of the given phenomena.

149. Analogy.

(i.) Relation to Enumerative Induction.

Analysis of content is necessary to establish relation.

The discussion in the last section has shown that an analysis of content is necessary to establish the connexion suggested by enumerative induction as subsisting between *S* and *P*, and which we are compelled by the principle of Identity (*see* § 17[1]) to think as not due to casual coincidence but to some sufficient ground. Now, the content of *S* is presented to us in the concrete particulars *a, b, c, d,* which we have observed to be both *S* and *P*. In these concrete particulars, therefore, we must look for this bond of union. The first step in the process is an inference by analogy, in which, as Dr. Bosanquet says, "we no longer count the examples, "but we weigh them" (*Logic*, vol. ii., p. 83). As it is the contents of *S* and *P* which are under consideration, the argument falls naturally into the second syllogistic figure (*see* § 113[2]), and may be symbolically represented thus—

The first step in analysis is inference by analogy; which falls into Fig. II.

$$\left.\begin{array}{l} P \text{ is } x\,R \\ S \text{ is } x\,R \end{array}\right\} \text{——} \left\{\begin{array}{l}\text{where } x \text{ is a common characteristic of} \\ a, b, c, d, \text{ and } R \text{ the unanalysed residue} \\ \text{of characteristics.}\end{array}\right.$$

$\therefore$ *S is probably P*——as the two possess a fundamental characteristic *x* in common.

Examples showing connexion of Analogy with Enumerative Induction.

The connexion with enumerative induction may be shown by a Concrete Example. Thus by Enumerative Induction we infer—

In several of the large towns in the north of England (*a, b, c, d*), the rate of infant mortality is very high (*P*),

Those same towns (*a, b, c, d*), are engaged in textile manufactures (*S*),

$\therefore$ The character of the industries of the towns (*S*) may be connected with the high infant mortality (*P*).

[1] First Edition, § 22. [2] First Edition, § 127.

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We must, if we wish to get any further than this mere suggestion of possibility, consider, not the number but, the character of the instances, and seek in that character some common feature which may give a reason for the connexion of S and P. This we find in the neglect of young infants consequent on their being frequently left for a considerable time without maternal care. Our argument from analogy will then run—

> When the rate of infant mortality is high (P), it is found upon examination that many infants have been neglected by their mothers (x),
>
> In textile manufacturing towns many mothers of young infants work in mills (S), and enquiry shows that the infants are then often neglected by them (x),
>
> Hence, there is a probable connexion between the character of the industry of the towns and the high rate of infant mortality, as in both cases we have the important common feature of maternal neglect, due to the working of the mothers in the mills.

Similarly, when the argument by enumerative induction given in the last section [*see* § 148 (ii.) (*a*)] is developed into an argument by analogy the common predicate is found in the fact that the animals quoted (polar bear, etc.) all need to escape observation, either in order to approach their prey, or to escape their enemies, and that the similarity of the colour of their coverings to that of the snow amidst which they live enables them to do this most easily. Thus, the probability of a universal connexion between the colour of the environment and that of the animals is strengthened.

Analogy need not be preceded by enumerative induction.

The argument by enumerative induction is thus seen to pass easily and naturally into one from analogy, immediately attention is turned from the number of the observed instances to their character. There is no necessity, however, for an argument from analogy to be approached through enumerative induction. When the instances presented to

observation offer immediately the characteristic marks on which we base the inference to the connexion of S and P, we can proceed at once to an inference from analogy, without any preliminary enumeration of the instances. In other words, whenever it is easy to do so, we commence the examination of content at once. Thus, *e.g.*, we infer immediately by analogy that rocks which bear the peculiar parallel marks which are termed *striæ*, have been at some former period subject to glacial action, even though no glaciers may now exist in the country in which they are found. Thus—

Rocks exposed to glacial action are striated,
Many rocks in Cumberland are striated,
∴ Cumberland has most probably been the scene of glacial action.

Many of the conclusions of geology, indeed, still rest upon nothing but analogy, *i.e.*, they are still hypotheses awaiting decisive proof.

Analogical origin of hypothesis as to nature of atoms.

As a further illustration of the suggestive value of analogy, we will take Clifford's account of the origin of Lord Kelvin's hypothesis as to the ultimate character of atoms. We must first premise that a 'vortex-ring' is a ring of matter with a rotatory motion of all its particles, the inside surface going one way and the outside surface the other way, as in a ring of smoke ejected by a smoker; and that a 'perfect liquid' would be one absolutely devoid of friction. "Helm-"holtz found, by a wonderfully beautiful calculation, that in "a perfect liquid where there is no friction it is impossible "for vortex motion to be generated or destroyed; in any part "of the liquid where there is no vortex motion no mechanical "action can possibly start it but where it once exists there "it is for ever, and no mechanical action can possibly stop it. "A vortex-ring may move from place to place; but it carries "with it the liquid of which it is composed, never leaving "any particle behind, and never taking up any particle from "the surrounding liquid. If we tried to cut it through with "a knife it would thin out like a stream of treacle, and the

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"thinner it got the faster it would go round; so that if we "multiplied together the number of revolutions in a second, "and the number of square millimetres in the cross-section "of the vortex-ring, we should always get the same product, "not only in all parts of the ring, but through all time. . . . "Here steps in Sir William Thomson with a brilliant con-"jecture. The ultimate atom of matter is required to be "indestructible, to have a definite mass, and definite rates of "vibration. A vortex-ring in a perfect liquid is inde-"structible, has a definite mass, and definite rates of vibra-"tion. Why should not the atom be a vortex-ring in a "perfect liquid? . . . The answer to this question is only "to be got at by examining further into the consequences "of the fundamental supposition, until either the desired "explanation of all phenomena is reached or some clear dis-"cordance with observed results shows that the whole hypo-"thesis is untenable" (Clifford, *Lectures and Essays*, pp. 166-7; *cf.* § 151 (ii.) (*b*) below).

(ii.) Logical Character.

A universal conclusion by Analogy would involve Undistributed Middle;

If we now examine the form of the argument from analogy, we find that to draw a universal conclusion would be to commit the fallacy of Undistributed Middle Term [*see* § 111 (ii.)[1]]. And, as the middle term is the bond of connexion between S and P, its undistributed character shows that that relation is not thoroughly established. To put it in another way. Our premises state that $x R$ is a consequence of both P and S, and from those consequences we want to infer identity of essential character in those two grounds. Now, if the propositions affirmed reciprocal relations and so could be simply converted, the universal conclusion would be permissible. For instance, if in the example from geology quoted above, it could be shown that the marks under consideration can be caused in no other possible way than by glacial action, the argument becomes demonstrative, for the first premise may be converted into 'Striation can be due to nothing but glacial action,' which is a true major premise to

[1] First Edition, § 125 (ii.).

an argument in the First Figure. But, in that case, we pass beyond analogy. It is because of this want of sufficiency of connexion that our conclusion from an inference by analogy can only be more or less probable; *i.e.*, it offers itself as a hypothesis which seems likely to be sufficient to explain the facts, but which requires further testing. Hence, there is no *proof* by analogy, but only a suggestion as to the direction in which proof may be found.

hence, such a conclusion can never be more than probable.

Regarded rather from its material than its formal side, analogy is seen to be an inference from partial identity of content to further identity of content. With Aristotle, it meant proportion, and the inference was of the character that *2:4=3:6*. From proportion of numbers, we pass to other proportions where the terms are not homogeneous, till we reach such relations as that x vibrations of air: $2x$ vibrations :: a note : its octave; or as that which establishes the relation between the number of vibrations of ether waves per second, and the resulting impression of colour; where, in the second series the idea of arithmetical ratio is altogether absent. Such statements of relation only mean that the series of physical stimuli and the series of corresponding impressions are so connected that from knowledge of the former we can infer the character of the latter. In this meaning of analogy the idea of number still lingers. In modern logic, as has been seen, analogy does not deal with quantitative relations, as such, but with considerations of content. The idea of analogy, then, was first modified so as to include all inferences from "resemblance of relations" (Whately, *Logic*, p. 323), and not simply those of quantity. In common usage, it denotes an inference from any kind of "resemblance," whether apprehended as a resemblance of relation or as one of quality, usually, however, confined to a pair of instances. Logically, this is still implicitly agreement of relations, as all qualities are at bottom forms of relation, though not necessarily analysed into them; and as 'resemblance' means partial identity. For, as Lotze says: "Similarity is always a mixture of "identity in one respect and difference in another. . . . We "cannot on the ground of the unanalysed similarity of two

Analogy originally meant proportion.

It now means argument from partial identity.

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"subjects transfer the predicate of one to the other, but "only on the ground of their demonstrated identity, identity "at least in respect of the conditions upon which the predi-"cate in question everywhere depends" (*Logic*, Eng. Trans., vol. i., pp. 327-8). There is, thus, implicit in an analogical argument, as its very basis, the assumption that such identity is present, or, as Lotze puts it: "It may be regarded as a "fundamental principle of analogy in the strict sense . . . that "of like things under like conditions like assertions are true" (*ibid.*, p. 322)—a principle which is evidently an expression of the law of identity. The same idea is expressed in Hegel's definition: "In the syllogism of Analogy we conclude from "the fact that some things of a certain kind possess a certain "quality, that the same quality is possessed by other things "of the same kind" (*Logic*, Eng. Trans., p. 326). It is because the element of identity is not explicit—*i.e.* formally, because the middle term is undistributed—that, as we saw above, the conclusion of our inference is only problematic; but the inference is only possible at all because an element of identity is assumed to be present. Analogy, then, in modern logic is practically equivalent to Aristotle's argument from example [*see* § 145 (i.)].

As this identity is implicit, the conclusion is only probably true.

(iii.) Criterion of Value.

Arguments from analogy are of different degrees of strength.

(*a*) *Importance is Teleological.* The arguments from analogy in common life may be—and often are—extremely superficial, and, on the other hand, such an argument may give a conclusion that very nearly approaches certainty. "It "would certainly be a very bad analogy to argue that since "the man Caius is a scholar, and Titus also is a man, Titus "will probably be a scholar too: and it would be bad because "a man's learning is not an unconditional consequence of his "manhood. Superficial analogies of this kind however are "very frequently met with. It is often argued, for example: "The earth is a celestial body, so is the moon, and it is "therefore in all probability inhabited as well as the earth. "The analogy is not one whit better than that previously "mentioned. That the earth is inhabited does not depend

"on its being a celestial body, but on other conditions, such "as the presence of an atmosphere, and of water in connexion with the atmosphere, etc.; and these are precisely "the conditions which the moon, so far as we know, does "not possess" (Hegel, *op. cit.*, p. 326). In contrast with this, the inference by analogy that at the poles of Mars are regions covered with ice and snow, is a very probable one; for "from the appearance and behaviour of those white spots "we infer that they have all the chemical and physical properties of frozen water" (Jevons, *Prin. of Sc.*, pp. 596-7). The force of an argument by analogy, therefore, depends upon the probability that the point of resemblance observed is the true ground of that inferred; in other words, that the two instances are really "like," *i.e.* have an essential point of identity. Now, this must not be known to be the case, or, as we saw in the last section, the inference has passed beyond the bounds of analogy, and become demonstrative. But, on the other hand, it must not be known not to be the case, for then there is no ground for inference at all. Hence, the strength of an argument from analogy depends upon the importance of the characteristics upon which it is based; upon whether the 'resemblance' contains an essential point of identity. Whenever the inference is fallacious it is because an accidental mark has been taken for an essential one.

The force of the argument depends on the importance of the marks on which it is based;

By what test, then, are essential characteristics to be distinguished from those which are accidental; in other words: What is the criterion of importance by which to judge the characteristics on which the inference is based? Now 'important' and 'essential' are relative terms, and point beyond themselves to something in relation to which the 'importance' and 'essentiality' is grounded. A characteristic is important for one purpose, but unimportant for another; and if its importance is so great that without it the former purpose cannot be achieved, we say it is essential. Essential, then, means 'essential for the end in view.' Thus, the criterion of value of marks for an analogical inference must be found in the conception of purpose or end. "Im-

and this is relative to purpose.

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"portance," says Dr. Bosanquet, "is relation to the purpose "or pervading nature, the 'import' of any system" (*Logic*, vol. ii., p. 94). This is easily seen when the cases with which an inference is concerned are purposive works of man. For example, by analogy we conclude that certain flints found in the earth are remains of weapons, because they bear marks of artificial shaping of such a kind as to adapt them to be cutting or piercing instruments, and corresponding, moreover, to those of flint weapons made and used by savages at the present day. Or, to take another example—

Electric bell-handles operate by being pressed,
The bell-handles in this house are made to be pressed,
∴ The house is probably hung with electric bells.

Similarly we should infer by analogy that a pair of spiked shoes were intended for use at cricket, because the affixing spikes to cricket shoes has an obvious purpose. In organic life, as we saw above [*see* § 144 (ii.)] the end or purpose is to be regarded as the perfection of nature reached through a long course of evolution. But inference here is often backwards, and then the nature is sought in the past. Thus, the biologist when his purpose is to attain knowledge of the common origin of divergent species of organisms seeks, as the ground of an inference from analogy, those marks which the course of evolution is most likely to have left largely unmodified; and they are not generally those which are most striking as points of resemblance.

The force of analogy does not depend upon 'ratio' of resemblance to difference,

(*b*) *Importance is not Amount of Resemblance.* From what has just been said it is evident that the force of an argument from analogy depends upon the character of the identity, and not upon the amount of similarity. As Mr. Sidgwick says: "Whenever 'degree' or 'amount' of resem-"blance or difference is spoken of, the student must re-"member that, for all purposes of reasoning, a resemblance "or difference is great or small, not according either to its "power of striking the observer's notice, or to the ***number*** of "'points' (or details) into which it may be analysed; but "according to the importance of its details in regard to the

"matter in hand." (*Process of Argument*, p. 194). But so firmly is the idea of enumeration rooted in many writers on logic—especially in those of the empiricist school—that this mistake is often made. Thus Mill says: "Since the value "of an analogical argument inferring one resemblance from "other resemblances without any antecedent evidence of a "connexion between them, depends on the extent of ascer-"tained resemblance, compared first with the amount of "ascertained difference, and next with the extent of the "unexplored region of unascertained properties; it follows "that where the resemblance is very great, the ascertained "difference very small, and our knowledge of the subject-"matter tolerably extensive, the argument from analogy "may approach in strength very near to a valid induction. "If, after much observation of *B*, we find that it agrees with "*A* in nine out of ten of its known properties, we may con-"clude with a probability of nine to one, that it will possess "any given derivative property of *A*" (*Logic*, III., xx., § 3). But, as we have seen throughout, we do not number but weigh our instances in analogy. And, moreover, how can we number "resemblances" or points in which *B* "agrees "with" *A*? Who shall decide as to whether a given point of identity or of difference is one "property" or a dozen? It is all a matter of arbitrary mental analysis. "Properties" are not isolated and separate individualities, which we can count and enumerate as we can balls or books. It is, therefore, meaningless to speak of a 'ratio' in this connexion; for a ratio requires a unit, and a unit is here unattainable. Moreover, it is impossible to estimate the number of "unknown" properties; such estimation assumes them known, and therefore to speak of "the extent of the unex-"plored region of unascertained properties" is to fall into a contradiction in terms. And it will be noticed that in his "ratio" Mill neglects these altogether, though he had insisted on their importance immediately before. His estimation of the amount of probability is, therefore, inconsistent with the doctrine he had himself just enunciated.

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as Mill held.

In accordance with the same view of numbering the attri-

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butes, Mill says that every point of difference furnishes a probability against the connexion of *S* and *P* (*cf. ibid.*, § 2). But, if the points of resemblance are essential, the points of difference may be disregarded; and similarly, if the points of difference are essential, no amount of resemblance in other points will make the inference a safe one. The utter hopelessness of inferring from the relative "extent of ascertained resemblance" and "extent of ascertained difference'—to say nothing of the presumably known extent of the unknown—is strikingly shown in the following passage from Dr. Wallace's *Darwinism*, in which he is speaking of the variations of plants, and in which it is seen that an analogical argument from amount of resemblance not only may lead an enquirer wrong when he is comparatively unacquainted with the subject-matter, but actually did mislead a great number of specialists. Dr. Wallace says: "All the "cucumbers and gourds vary immensely, but the melon "(Cucumis melo) exceeds them all. A French botanist, "M. Naudin, devoted six years to their study. He found "that previous botanists had described thirty distinct species, "as they thought, which were really only varieties of melons. "They differ chiefly in their fruits, but also very much in "foliage and mode of growth. Some melons are only as "large as small plums, others weigh as much as sixty-six "pounds. One variety has a scarlet fruit. Another is not "more than an inch in diameter, but sometimes more than "a yard in length, twisting about in all directions like a "serpent. Some melons are exactly like cucumbers; and "an Algerian variety, when ripe, cracks and falls to pieces, "just as occurs in a wild gourd" (pp. 87-8).

An inference drawn from mere resemblance is often fallacious.

(iv.) Confirmation by Negations.

Inference by analogy is confirmed when differences are found to be unessential to the end in view,

Differences, as we have said, cannot override essential identity. But, in so far as our instances differ, these differences call for explanation, and when this explanation shows that they are not essential, but merely accidental, in relation to the purpose in view, the argument from analogy is confirmed; though it must be borne in mind, that this confirma-

tion can never amount to demonstration whilst the inference remains analogical. Thus, in the example just quoted, the striking differences of the different varieties were shown on investigation not to be essential; *i.e.* they were proved to be due to the course of evolution and not to the original common nature which was the subject of investigation.

This confirmation by negative instances would reach its most perfect stage when "they should form a single ana-"logical inference, in which each positive premise and the "positive conclusion should be *materially* defined and limited "by the corresponding negative judgment . . . that is to "say, every judgment *A is B* would be supported by its con-"verses *Not-A is Not-B,* and *Not-B is Not-A*" (Bosanquet, *Logic*, vol. ii., p. 105). When these are fully established the relation between *A* and *B* is proved to be both universal and reciprocal. The inference has then passed beyond analogy, and become demonstrative. Hence, analogy suggests not merely hypotheses but also the lines along which their confirmation should be sought. Even when the conclusion is not confirmed, an examination of negative instances is of great use in pointing out the direction in which amendment should be sought. For example, to return to one of our earlier examples [*see* (i.) above, and § 148 (ii.) (*a*)] the conclusion drawn was that there was a probability that 'Animals living amongst snow have white coverings.' If we now seek the negative instances, this conclusion would be confirmed if we found that, on the one hand, 'No animals which live amongst the snow have coverings other than white' and on the other hand, that 'No animals with coverings other than white live amongst the snow.' But these statements experience does not bear out. For the "sable retains its rich brown fur "throughout the Siberian winter," the musk-sheep, which is also an arctic animal, is brown, and the raven remains black, though it is "found even in mid-winter as far north as any "known bird or mammal" (Wallace, *op. cit.*, p. 191). On the other hand, sheep, horses, and other animals are found white in other than arctic regions. The conclusion at which we arrived was, therefore, a false one, in that it was stated too

but the full carrying out of this takes us beyond analogy.

generally. But, the correction is to be found in the ground from which we drew that conclusion—viz., the need to escape observation. For examination of the exceptions quoted shows that this need for escaping observation to avoid danger does not exist in the case of the musk-sheep, which is gregarious, nor in that of the raven, on account of its strength; whilst the sable lives in winter amongst trees and resembles their bark in colour. Thus, if we modify the previous conclusion to 'Arctic animals which need concealment have coverings agreeing in colour with their surroundings,' we shall find it confirmed by our negative instances, and further examination will show that the same rule can be extended to the animals of all districts.

But in this consideration of negative instances we are really passing beyond the limits of real analogical inference, for our analysis of content is more thorough than that on which analogy is based, and is going beyond the suggestion of hypotheses to their proof.

CHAPTER IV.

DEVELOPMENT OF HYPOTHESES.

150. Nature of Hypotheses.

(i.) Formation of Hypotheses.

Hypotheses are necessary to all scientific enquiry.

We have traced the development of hypotheses from their first appearance under the guise of the general names which give the first suggestion for enumerative induction to the point at which they form the starting-point of exact scientific enquiry. Nor can such enquiry have any other starting-point; for science can never grow out of observations made at random, nor is experiment ever performed with no object in view. Every scientific enquiry is an attempt to establish a relation between two elements of reality, but before such an attempt can be made it is obvious that the connexion in question must have been already thought as possible; no one can ever attempt to prove what has never occurred to him. And the supposition of a possible relation is a hypothesis. Even Mill grants this, and speaks of the function of hypotheses in "suggesting observations and experiments" as "one which must be reckoned absolutely indispensable in "science. . . . Without such assumptions, science could "never have attained its present shape: they are necessary "steps in the progress to something more certain; and nearly "everything which is now theory was once hypothesis" (*Logic*, III., xiv., § 5). Indeed, a little later Mill gives a sketch of the inductive process, quite at variance with the empirical method set forth in the earlier part of the book, but much nearer the truth. He says: "The process of "tracing regularity in any complicated, and at first sight "confused set of appearances, is necessarily tentative we

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"begin by making any supposition, even a false one, to see "what consequences will follow from it; and by observing "how these differ from the real phenomena, we learn what "corrections to make in our assumption. The simplest "supposition which accords with the more obvious facts, is "the best to begin with; because its consequences are the "most easily traced. This rude hypothesis is then rudely "corrected, and the operation repeated; and the comparison "of the consequences deducible from the corrected hypo-"thesis, with the observed facts, suggests still further correc-"tion, until the deductive results are at last made to tally "with the phenomena" (*ibid.*). As all reality is "compli-"cated and at first sight confused," this method is applicable to all scientific enquiry; and thus hypotheses must guide all attempts to attain knowledge.

All circumstantial evidence is the testing of a hypothesis.

And this is the case, not only in enquiries into the phenomena of physical nature, but equally so with any investigation into past events, when the testimony of direct observation is unavailable. Thus the weighing of all circumstantial evidence is a case of procedure by hypothesis. A crime has been committed and an individual is suspected. His guilt is then the hypothesis. From this hypothesis certain deductions can be made—his presence at the place at the time, his possession of a certain weapon, his leaving certain marks behind him, and so on, and from the coincidence of these with fact the hypothesis is proved or rejected. Or take an enquiry into a case of anonymous authorship. A hypothesis is formed that the author was a certain individual. Then from this hypothesis it is deduced that certain peculiarities of manner and style may be looked for in the work; if these are found they so far support the hypothesis. If no other peculiarities are discovered than those which also occur in acknowledged works of the supposed author, then the hypothesis is greatly strengthened. And lastly, if all the noticed peculiarities of the assumed author appear in the anonymous work, the hypothesis may be held to be practically established. A similar line of enquiry is that by which the order in which Shakespeare's plays were written is deter-

mined by internal evidence. A play exhibits certain features of verse construction and style, a certain proportion of rhyming lines, certain historical references, and so on, all of which can be accounted for on the hypothesis that it was written at a certain definite period, but are otherwise inexplicable. For example, in speaking of the date of the '*Two Gentlemen of Verona*' Mr. Gollancz says: "The follow-"ing general considerations place it among the earliest of "Shakespeare's productions, *i.e. circa* 1590-1592:—the sym-"metrical arrangement of the characters; the unnaturalness "of some of its incidents, especially the abrupt *dénouement;* "the finely finished regularity of the blank verse, suggestive "of lyrical rather than of dramatic poetry, and recalling the "thoughts and phraseology of the sonnets; . . . the alter-"nate rhymes; the burlesque doggerel; the quibbles; and "the fondness for alliteration" (*Preface to Temple Edition*).

Every hypothesis must be based on facts.

But, whatever the subject-matter, the hypothesis must be drawn from the facts whose explanation is sought. The enquirer does not start with an assumption which he knows to be false—as Mill seems to suggest, though he says a few lines later that the first hypothesis "accords with the most "obvious facts"—but with that which seems to offer the most plausible explanation of the facts. And he is careful to ascertain as exactly as possible what the facts are whose explanation he seeks. As Whewell points out: "A vague "and loose mode of looking at facts very easily observable, "left men for a long time under the belief that a body, ten "times as heavy as another, falls ten times as fast;—that "objects immersed in water are always magnified, without "regard to the form of the surface;—that the magnet exerts "an irresistible force;—that crystal is always found asso-"ciated with ice;—and the like. These and many others are "examples how blind and careless men can be, even in obser-"vation of the plainest and commonest appearances; and "they show us that the mere faculties of perception, although "constantly exercised upon innumerable objects, may long "fail in leading to any exact knowledge" (*Novum Organon Renovatum*, p. 61).

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No rules can be given for the invention of hypotheses.

It being, then, first ascertained, as exactly as possible, what is to be accounted for, analogy with other known phenomena suggests hypotheses. No rules can be given here. The great scientific investigator is distinguished from other men just by that sagacity which enables him to imagine possible explanations, to see their consequences, and finally to select out of the several suggestions that have occurred to his mind, the one which finally does lead to a full explanation of the facts. This process may go on rapidly or slowly; sometimes a false hypothesis is dwelt on and its consequences worked out in detail before its inadmissibility becomes apparent, at others it is rejected almost as soon as formed.

Wrong hypotheses generally precede right ones.

But the general process is the same; wrong hypotheses generally precede right ones; and that frequently in the same mind. As Whewell remarks, "To try wrong guesses is, "with most persons, the only way to hit upon right ones" (*op. cit.*, p. 79). Of course, it is not pure unguided guess-work. In most cases the attempts of previous enquirers have shown more or less plainly in what direction explanation must be sought; either by the partial establishment of some hypothesis, or by making manifest the inadmissibility of others. Facility in framing hypotheses cannot, then, be reduced to rule, and hence falls outside the province of logic.

Every hypothesis should be clearly apprehended, and held subject to revision.

But rules can be given as to how they should be framed, and how they should be dealt with when formed. In the first place, every hypothesis must be clearly apprehended; and in the second place, it must be held subject to revision. The truly scientific thinker has none of the spirit which says 'If the facts 'do not agree with the theory, so much the worse for the facts, It was against this spirit—this assumption of hypotheses on little or no evidence, and obstinate adherence to them, this attempt to explain the world *à priori* without much direct reference to the world—that Newton protested when he said *hypotheses non fingo* [*see* § 145 (iv.)]. On the contrary, to the true thinker, as Brown says, hypotheses "are of use . . . not "as superseding investigation, but as directing investigation "to certain objects,—not as telling us what we are to believe, "but as pointing out to us what we are to endeavour to

"ascertain" (*Philosophy of the Human Mind*, vol. i., pp. 230-1). On this point we cannot do better than quote an admirable passage from Whewell. He says: "But if it be an advan- "tage for the discoverer of truth that he be ingenious and "fertile in inventing hypotheses which may connect the "phenomena of nature, it is indispensably requisite that he "be diligent and careful in comparing his hypotheses with "the facts, and ready to abandon his invention as soon as it "appears that it does not agree with the course of actual "occurrences. This constant comparison of his own concep- "tions and supposition with observed facts under all aspects, "forms the leading employment of the discoverer: this "candid and simple love of truth, which makes him willing "to suppress the most favourite production of his own in- "genuity as soon as it appears to be at variance with realities, "constitutes the first characteristic of his temper. He must "have neither the blindness which cannot, nor the obstinacy "which will not, perceive the discrepancy of his fancies and "his facts. He must allow no indolence, or partial views, or "self-complacency, or delight in seeming demonstration, to "make him tenacious of the schemes which he devises, any "further than they are confirmed by their accordance with "nature. The framing of hypotheses is, for the enquirer "after truth, not the end, but the beginning of his work. "Each of his systems is invented, not that he may admire it "and follow it into all its consistent consequences, but "that he may make it the occasion of a course of active ex- "periment and observation. And if the results of this pro- "cess contradict his fundamental assumptions, however in- "genious, however symmetrical, however elegant his system "may be, he rejects it without hesitation. He allows no "natural yearning for the offspring of his own mind to draw "him aside from the higher duty of loyalty to his sovereign, "Truth: to her he not only gives his affections and his wishes, "but strenuous labour and scrupulous minuteness of atten- "tion" (*op. cit.*, pp. 80-1).

Examples of Kepler

As striking examples of the true scientific spirit, Whewell quotes Kepler and Newton. The former not only most

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earnestly and carefully worked out the results of his various hypotheses as to the orbit of Mars, but "never allowed the "labour he had spent upon any conjecture to produce any "reluctance in abandoning the hypothesis, as soon as he had "evidence of its inaccuracy" [*ibid.*, and *cf.* § 147 (ii.) (*c*) above].

and Newton.

A similar spirit was shown by Newton in respect to his hypothesis that the moon is retained in her orbit by the force of gravity. From this hypothesis, he calculated that the moon ought to be deflected from the tangent of its orbit something more than fifteen feet every minute. But the apparent deflection was only thirteen feet. This discrepancy, comparatively small though it was, Newton accepted as a disproof of his hypothesis, and "laid aside at that time "any further thoughts of this matter." But some fifteen years later, the distance of the moon from the earth had been more exactly ascertained; and Newton repeated his calculations, working with these new values. The agreement between the calculated and the normal actual deflection was then seen to be remarkably precise, and the hypothesis became an established theory.

(ii.) Kinds of Hypotheses.

Erroneous hypotheses may be of value in guiding investigation.

(*a*) *Working and Descriptive Hypotheses.* All hypotheses then, so long as they remain hypotheses—*i.e.* are not fully proved—must be held subject to revision, modification, or even rejection. But it does not follow that a hypothesis which is finally disproved has been of no service. Every well imagined hypothesis, suggested by a real knowledge of the facts dealt with, opens out a line of enquiry which is rarely fruitless.

A *Working Hypothesis* is one of a descriptive character, and accepted provisionally as a guide to enquiry.

Such a *Working Hypothesis* is frequently provisionally assumed as furnishing a description of the facts and collecting them together, when knowledge is not sufficiently advanced to admit of a true explanatory hypothesis being proposed. A working hypothesis, then, is of a descriptive character, and is more or less provisional. As Jevons says: "When [Professor Huxley] advocates the use "of 'working hypotheses' he means no doubt that any

"hypothesis is better than none, and that we cannot avoid "being guided in our observations by some hypothesis or "other" (*Prin. of Sc.*, p. 509). Such a hypothesis was that which spoke of the "electric fluid," when the term 'fluid' was used as merely a convenient way of gathering together and describing some of the known phenomena of electricity, but with a full consciousness that electricity is not a fluid in the correct meaning of that word.

Many descriptive hypotheses which were really working hypotheses, though their provisional character was not recognized at the time, have been of great use in the advancement of knowledge. As an example we may take the Ptolemaic hypothesis of cycles and epicycles to account for the motions of the sun, moon, and planets. This hypothesis enabled the ancient astronomers to calculate those motions with considerable accuracy, and was, therefore, "of "immense value to the progress of astronomical science; for "it enabled men to express and reason upon many important "truths which they discovered respecting the motion of the "stars, up to the time of Kepler" (Whewell, *op cit.*, p. 84). Yet the hypothesis was essentially false as an explanation; for it assumed the earth to be the centre round which all heavenly bodies revolve, and the motions of all such bodies to be circular. Of such descriptive hypotheses more than one may for a time appear to be equally true, *i.e.* the facts in question may be *described* in more than one way. Many of the observed facts of the motions of the heavenly bodies could, for example, be as accurately described on the Cartesian hypothesis of vortices as on the Newtonian theory of gravitation. But, as other facts were accurately observed and measured, the hypothesis of vortices required a continual series of modifications each of which made it more complex than before, whilst the Newtonian theory was found to explain these new facts as well as those for whose interpretation it had been originally suggested. Thus, the necessity of relating the original facts to other series of phenomena furnishes a test of the truth of a descriptive hypothesis, and as it is only in such relation that explanation can be found,

Several descriptive hypotheses may for a time appear equally true;

but a false hypothesis cannot lead to explanation.

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so only a descriptive hypothesis which ultimately admits of explanation can be true.

Hypotheses of Law and of Cause differ only in degree.

(*b*) *Hypotheses of Law and Hypotheses of Cause.* This leads us to the difference sometimes drawn between hypotheses of law and hypotheses of cause. Some logicians confine the term hypothesis to the latter class of suppositions. Thus, Ueberweg defines a hypothesis as "the pre-"liminary admission of an uncertain premise, which states "what is held to be a cause, in order to test it by its conse-"quences" (*Logic*, Eng. Trans., p. 505). When this distinction is drawn, the cause is generally regarded as external to the phenomena to be explained, whilst the law is the expression of relations holding within that series of phenomena. But as every series of phenomena selected for explanation is arbitrary in its limits, as even its full description can be found only in its relation to other phenomena, and as it is the totality of those conditions which is its cause, and a knowledge of which furnishes its explanation, so it does not appear that for logic there is any fundamental distinction between hypotheses of law and hypotheses of cause. A hypothesis of law selects some special aspect of some definite phenomena. As Mach says: "A rule, reached by the "observation of facts, cannot possibly embrace the *entire* "fact, in all its infinite wealth, in all its inexhaustible mani-"foldness; on the contrary, it can furnish only a rough "*outline* of the fact, onesidedly emphasizing the feature that "is of importance for the given technical (or scientific) end "in view" (*Science of Mechanics*, Eng. Trans., pp. 78-9). This selected aspect is then investigated carefully so that an exact and quantitative expression of the relation involved may be attained, and so the phenomena be thought with the greatest possible precision. "The business of physical "science is the reconstruction of facts in thought, or the "abstract quantitative expression of facts. The rules which "we form for these reconstructions are the laws of nature" (Mach, *op. cit.*, p. 502). A hypothesis of cause is only a fuller development of this, and grows out of it. "In the "conviction that such rules are possible lies the law of

A hypothesis of law deals with a relation between known phenomena,

"causality. The law of causality simply asserts that "the phenomena of nature are *dependent* on one another" (*ibid.*). Every hypothesis of law which exactly states a relation between certain phenomena is, *ipso facto*, a hypothesis of cause, for it states fully the essential conditions for the occurrence of some definite element of reality. Of course, many laws would be necessary to express the cause of any concrete fact in all its complexity; for every detail is the expression of some law. But the cause of any mode of action, or of any determinate aspect of the complex fact, may be much more simply expressed. For example, the theory of gravitation is a theory as to cause, when regarded in relation to facts of motion determined by it, as it affirms the essential conditions of such movements; it is also a statement of law, as it expresses exactly the character and amount of that motion. The enquiry into the cause of gravity itself is evidently the attempt to push explanation yet further back, and to reach a knowledge of the essential and ultimate constitution of matter, and the relations of its permanent properties to each other. In many cases a hypothesis as to law cannot be framed without making at the same time a suggestion of cause, and thus it is impossible to obey Comte's dictum that "Science must study only the laws of phenomena, "and never the mode of production," for the law cannot be separated from the mode of production. For example, the undulatory theory of light involves the hypothesis of a luminiferous ether, without which the laws of optics could not be intelligibly expressed. Similarly, all geological laws necessarily involve a reference to the origin of the phenomena with which the science deals. The distinction between hypotheses of law and of cause is, then, only one of degree. The former are descriptive statements of the exact character of the phenomena to be explained, when its relations to other phenomena are not in question, and thus naturally precede the latter, in which an attempt is made to formulate those relations. Further, hypotheses of cause are also statements of law, for they assume a mode of relation between phenomena. But in no case can a law be the ex-

and a hypothesis of cause with the totality of relations.

pression of a mere *empty* relation; it must be a relation between elements of reality. Thus, law must, in every case, involve a reference to an agent; and, on the other hand, the assumption of an agent can never be kept entirely independent of its relations to other things. In so far, then, as a hypothesis deals with the relations between known phenomena, it is a hypothesis of law; in so far as it goes beyond them, and assumes a necessary relation between the given phenomena and other elements of reality, as yet unknown, or at least not known to be in essential relation with them, it is a hypothesis of cause. Finally, when established, every law takes its place as an element in an explanatory theory, and an explanatory theory is one which expresses the full ground for the phenomena, and that full ground is the true cause [*cf.* § 144 (i.)].

151. Employment of Hypotheses.

(i.) Newton's Rules of Philosophizing.

Statement of Newton's Rules of Philosophizing.

(*a*) *Statement of the Rules.* Various attempts have been made to formulate conditions which must be satisfied by every hypothesis before it be even provisionally accepted. The most famous of these are the "Rules of Philosophizing" given by Newton at the beginning of the Third Book of the *Principia*. They are as follows:—

"**Rule I.** No more causes of natural things are to be "admitted than such as are both true, and sufficient "to explain the phenomena of those things.

"**Rule II.** Natural effects of the same kind are to be "referred as far as possible to the same causes.

"**Rule III.** Those qualities of bodies that can neither "be increased nor diminished in intensity, and which "are found to belong to all bodies within the reach "of our experiments are to be regarded as qualities "of all bodies whatever.

"**Rule IV.** In experimental philosophy propositions "collected by induction from phenomena are to be "regarded either as accurately true or very nearly

"true, notwithstanding any contrary hypotheses, till "other phenomena occur, by which they are made "more accurate, or are rendered subject to exceptions."

Rule I.:

(*b*) *Examination of the Rules*—**Rule I.** Of these rules, the first is the most important, and has provoked much discussion. What exactly does Newton mean by a *vera causa* —a true cause? The answer generally given is that it is a cause independently known to exist. Sometimes it is said, as, for instance, by Ueberweg, that it is one which has been "already shown to exist as an actual power in Nature" (*op. cit.*, p. 511), but this restriction certainly cannot be justified. As De Morgan says: "The physical philosopher "has frequently to conceive law which never was in his "previous thought—to educe the unknown, not to choose "among the known" (*Budget of Paradoxes*, p. 51). Mill holds the meaning to be "that the cause, though not known "previously, should be capable of being known thereafter; "that its existence should be capable of being detected, and "its connexion with the effect ascribed to it should be "susceptible of being proved, by independent evidence" (*Logic*, III., xiv., § 4). Similarly, Herschel says *veræ causæ* "must be such as we have good inductive grounds to believe "do exist in nature, and do perform a part in phenomena "analogous to those we would render an account of; or "such, whose presence in the actual case can be demonstrated "by unequivocal signs" (*Nat. Phil.*, § 209). The evidence to be required is not that of direct perception. To demand, indeed, that the agent assumed should be open to direct perception would be to exclude many of the most fruitful hypotheses of modern science, *e.g.* the atomic theory of the constitution of matter, and the undulatory theory of light with its assumption of a luminiferous ether. Herschel, apparently, reduces the rule to a requirement that the hypothesis assumed shall be sufficient to account for the phenomena, and shall be suggested by analogy, after a careful examination of the facts. And this is also the sense in

A *Vera Causa* is generally said to be a cause independently known,

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which Whewell accepts it, when he says that *veræ causæ* are "not those which are collected in a loose, confused and precarious manner, by undisciplined minds, from obvious "phenomena, but those which are justly and rigorously "inferred" (*Phil. of Discovery*, p. 189), and he regards it as valuable as expressing "one of the most important tests "which can be given of a sound physical theory" (*ibid.*, p. 190), viz., the consilience of inductions [*see* § 145 (vi.)]. Therefore he says: "Newton's Rule, then, to avoid mistakes, "might be thus expressed: That we may, provisorily, assume "such hypothetical cause as will account for any given class "of natural phenomena; but that when two different classes "of facts lead us to the same hypothesis, we may hold it to "be a *true cause*" (*ibid.*, p. 192). But this demand for independent evidence seems to be merely an outcome of the enumerative view of induction, and to have no remote connexion with the doctrine of chances. There is also, throughout all, a manifest separation of the cause from its effect, a regarding it as something external to the phenomena which it determines, as an agent which by its own power, and irrespective of the nature of the object acted on, produces certain results. Taking the view that cause is the totality of conditions, we must hold that a *vera causa* is simply that complex of conditions which alone avoids contradiction in our thought, that is, which alone enables us to think the phenomena as part of systematic reality. As Dr. Bosanquet puts it: "A *vera causa* . . . is a thing, or "occurrence in a thing, whose reality we are thoroughly "convinced of from the necessity of reconciling observed "data, and there is no reason in the nature of things why "a single science or a single range of reality should not "suffice to produce such conviction" (*Logic*, vol. ii., p. 159). Of course, every hypothesis which is proved to be true, is a hypothesis with a *vera causa*. Whatever we regard as a *vera causa*, we regard as necessary in all its details, if not to the production of the phenomena with which we are more immediately concerned, yet for that of some other phenomena. We regard it, in short, as part of reality and

but independent evidence is not essential.

A *Vera Causa* is one whose assumption is necessary to reconcile observed data.

in real and necessary relation with the facts we are considering. Book V. Ch. IV.

Rule II. Newton's Second Rule is doubtless valid, but, as Whewell says, "The question is, *Are* the effects of the "same kind? This once settled, there will be no question "about the propriety of assigning them to the same cause" (*op. cit.*, p. 193). This can only be ascertained by a careful analysis of the facts and comparison of them with the conclusions rigorously deduced from a hypothesis clearly conceived. "Thus it does not appear that this Rule of Newton "can be interpreted in any distinct and positive manner, "otherwise than as enjoining that, in the task of induction, "we employ clear ideas, rigorous reasoning, and close and "fair comparison of the results of the hypothesis with the "facts" (*ibid.*, p. 194). Rule II.

Rule III. Newton's Third Rule can scarcely be accepted as it stands. The extension of qualities to unobserved bodies cannot be justified by simple enumeration. It is only the demand of thought—the impossibility of conceiving the universe as a unity without such extension—that will justify it in any case. "The Rule," says Whewell, "cannot "be applied without attempting to decide, by the casual "limits of observation, questions which necessarily depend "upon the relations of ideas" (*ibid.*, p. 196). Rule III.

Rule IV. Newton's Fourth Rule is valuable as pointing out the necessity of careful verification, and of needful modification of hypotheses; and also as indicating the value of mere working hypotheses. Rule IV.

(ii.) Conditions of a Valid Hypothesis.

(*a*) *Statement of Conditions.* Every hypothesis is an attempt to find meaning in observed phenomena, to constitute reality in a rational way. It follows that the fundamental condition of a valid hypothesis is that it should explain and give meaning to the facts of observation. And it can only do this if it embraces those facts in that systematic whole which is the one form under which it is possible to think the universe. This general condition, A good hypothesis gives a meaning to observed facts.

then, may be considered as involving three subordinate conditions :—

(1) That the hypothesis be self-consistent, and in harmony with all other laws included in the conceived system of reality.
(2) That it furnish a basis for rigorous deductive inference of consequences.
(3) That these inferred consequences be in agreement with reality.

Of these conditions, the first two are applicable to the formation of every hypothesis, no matter how provisional a character it may have; the third is a condition of the acceptance of a hypothesis as true. These conditions are practically the same as those enunciated by Jevons in his *Principles of Science* (p. 511), and are in substantial agreement with those set forth by Herschel in his *Discourse on Natural Philosophy*, where he requires that the hypothesis "shall render a rational "account of any natural phenomena," that its consequences shall be followed out, and compared "with all the particular "cases within our knowledge" (§§ 209, 210).

A hypothesis must be self-consistent and agree with received laws.

(*b*) *Examination of Conditions.* **First Condition.** The requirement that a hypothesis do not involve consequences which would be inconsistent with itself, and that it be not in conflict with any other received law is obviously necessary, as the very aim of enquiry is to obtain a more consistent and systematic understanding of reality. Of course, hypotheses which were inconsistent with conceptions of nature previously held, have been not only formed but proved, as *e.g.* when the Copernican theory of the heavens was substituted for the Ptolemaic. But in every such case the new hypothesis is not held concurrently with theories inconsistent with itself; on the contrary, the previously accepted theories are modified and brought into harmony with the new view necessitated by the fuller study of reality. As science advances, the probability that necessity will arise for any such thorough-going revision of our concept of any aspect of reality becomes continually smaller.

Difficulty of conception is no objection to a hypothesis.

Mere difficulty of conception, however, as Jevons points out, is not a bar to the acceptance of a hypothesis. Many of the best approved theories of modern science present considerable difficulties. That gravity should act through space not filled with any medium for its operation, and that its power should be exercised in absolute disregard of intervening obstacles are such difficulties. Nor is it easier to conceive the luminiferous ether demanded by the undulatory theory of light. "We are asked by physical philosophers," says Jevons, "to give up our presuppositions, and to believe "that interstellar space which seems empty is not empty at "all, but filled with *something* immensely more solid and "elastic than steel. As Young himself remarked, 'the "luminiferous ether, pervading all space, and penetrating "almost all substances, is not only highly elastic, but abso-"lutely solid!!!' Herschel calculated the force which may "be supposed, according to the undulatory theory of light, "to be constantly exerted at each point in space, and finds it "to be 1,148,000,000,000 times the elastic force of ordinary "air at the earth's surface, so that the pressure of ether per "square inch must be about seventeen billions of pounds. "Yet we live and move without appreciable resistance "through this medium, immensely harder and more elastic "than adamant. All our ordinary notions must be laid "aside in contemplating such an hypothesis; yet it is no "more than the observed phenomena of light and heat force "us to accept" (*Prin. of Sc.*, pp. 515-6).

A hypothesis must give a basis for deductive inference.

Second Condition. It is impossible to infer any consequences from the absolutely unknown. As Jevons says: "We can only infer what would happen under supposed con-"ditions by applying the knowledge of nature we possess to "those conditions" (*op. cit.*, pp. 511-2). Hence, our hypothesis must be always in accordance with some analogy, or based on some experience; otherwise we can draw no conclusions from it. To again quote Jevons: "When we "attempt to explain the passage of light and heat radiations "through space unoccupied by matter, we imagine the exist-"ence of the so-called *ether*. But if this ether were wholly

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"different from anything else known to us, we should in "vain try to reason about it. We must apply to it at least "the laws of motion, that is, we must so far liken it to "matter. And as, when applying those laws to the elastic "medium air, we are able to infer the phenomena of sound, "so by arguing in a similar manner concerning ether we are "able to infer the existence of light phenomena correspond-"ing to what do occur. All that we do is to take an elastic "substance, increase its elasticity immensely, and denude it "of gravity and some other properties of matter, but we must "retain sufficient likeness to matter to allow of deductive "calculations" (*op. cit.*, p. 512).

This condition involves Newton's first two rules.

It was to this necessity of securing that hypotheses should not be mere arbitrary fictions that we may attribute the first two of Newton's Rules of Philosophizing; the condition we are discussing demands a "true cause" in the only sense in which the requirement can be admitted. It is on the ground of the violation of this condition, that Clifford objects to Lord Kelvin's hypothesis that an atom is a vortex-ring in a perfect liquid [*cf.* § 149 (i.)]. He says: "A true explanation "describes the previous unknown in terms of the known; "thus light is described as a vibration, and such properties of "light as are also properties of vibrations are thereby ex-"plained. Now a perfect liquid is not a known thing, but a "pure fiction. The imperfect liquids which approximate to "it, and from which the conception is derived, consist of a "vast number of small particles perpetually interfering with "one another's motion. This molecular structure not only "explains the fact that they behave like perfect liquids when "at rest, but also makes it necessary that they should not "behave like perfect liquids when in motion. Thus a liquid "is not an ultimate conception, but is explained—it is known "to be made up of molecules; and the explanation requires "that it should not be frictionless. The liquid of Sir "William Thomson's hypothesis is continuous, infinitely "divisible, not made of molecules at all, and is absolutely "frictionless. This is . . . a mere mathematical fiction" *Lectures and Essays*, p. 169).

Another essential of a hypothesis from which rigorous deductive inferences can be drawn is that it shall be conceived with perfect clearness and definiteness. If it relates to phenomena capable of quantitative measurement it must be expressed with mathematical exactness, so that the precise amount of its results can be calculated. These conditions, we have seen, Newton fulfilled with the hypothesis of gravity [*see* § 150 (i.)]. This also is the case with the laws of physics, which are, indeed, more exact than the observations or experiments by which they are tested, because of the imperfection which attends all actual physical measurements.

A hypothesis must be clear and definite.

Third Condition. As the very aim of every hypothesis is to express the relations which exist in reality, it is obviously essential that the hypothesis should be verified by comparison of the results deduced from it with facts of observation. In order that this may be done it is essential that the consequences of the hypothesis should be inferred with the utmost precision, and that the comparison of these consequences with the facts should be made with great care and accuracy. The hypothesis must, of course, in the first place agree with the phenomena it was invented to explain. But we must not rest satisfied with this. It must be compared with facts of the greatest possible variety, and exemplifying every possible case which can be brought under the hypothesis. A single absolute disagreement with facts is fatal to a hypothesis. Nevertheless, a hypothesis is not to be hastily abandoned on the first *prima facie* conflict with reality. It must be made clear first that the opposing facts have been rightly grasped—that they really are in conflict with the hypothesis. And if this turns out to be the case, enquiry should then be made as to whether they have not been partly determined by the existence of interfering conditions, in the absence of which they would be found in agreement with the hypothesis. And, finally, if, after making all such allowances, the facts are still in conflict with the hypothesis, it must be considered whether a modification of the latter will meet the case, or whether it must be absolutely rejected.

The consequences inferred from a hypothesis must agree with reality.

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(iii.) Extension of Hypotheses.

The existence of new facts may be deduced from a true hypothesis.

If the hypothesis is true, it will generally be possible to infer deductively from it facts which have not been before explained or which have even been unobserved. Though Comte's saying that "Prevision is the test of "true theory" cannot be accepted absolutely, as a hypothesis may be true even though it does not enable the discovery of new facts to be predicted, yet it is, doubtless, one of the tests of the validity of a hypothesis. As Whewell says: "When the hypothesis, of itself and without "adjustment for the purpose, gives us the rule and reason of "a class of facts not contemplated in its construction, we have "a criterion of its reality, which has never yet been pro-"duced in favour of falsehood" (*Nov. Org. Ren.*, p. 90). The history of science is full of such extension and prediction.

Examples from gravitation,

Thus, for example, the discovery of Neptune was predicted by deductive reasoning from the principle of gravitation. Other instances of deductions from the same theory are thus summarized by Whewell: "The attraction of the sun "accounted for the motions of the planets; the attraction "of the planets was the cause of the motion of the satellites. "But this being assumed, the perturbations, and the motions "of the nodes and aphelia, only made it requisite to extend "the attraction of the sun to the satellites, and that of the "planets to each other;—the tides, the spheroidal form of "the earth, the precession, still required nothing more than "that the moon and sun should attract the parts of the earth, "and that these should attract each other;—so that all the "suppositions resolved themselves into the single one, of the "universal gravitation of all matter" (*op. cit.*, p. 92).

from the undulatory theory of light,

The undulatory theory of light has led to a great wealth of anticipations. The anticipation of the phenomena of circular polarization by Fresnel is thus described by Whewell—"Having ascertained by observation that two "differently-polarized rays, totally reflected at the internal "surface of glass, suffer different *retardations* of their undu-"lations, he applied the formulæ which he had obtained for

"the polarizing effect of reflection to this case. But in this "case the formulæ expressed an impossibility; yet as algebraical formulæ, even in such cases, have often some meaning, 'I interpreted,' he says, 'in the manner which appeared "to me most natural and most probable, what the analysis "indicated by this imaginary form'; and by such an interpretation he collected the law of the difference of undulation of the two rays. He was thus able to predict that by "two internal reflections in a *rhomb*, or parallelopiped of "glass, of a certain form and position, a polarized ray would "acquire a circular undulation of its particles; and this "constitution of the ray, it appeared, by reasoning further, "would show itself by its possessing peculiar properties, "partly the same as those of polarized light, and partly "different. This extraordinary anticipation was exactly "confirmed" (*Hist. of Ind. Sciences*, vol. ii., p. 357).

from the tides,

Another example is Whewell's own prediction that as the tides of the German Ocean consist of interfering tidal waves, one coming round the North of Scotland and the other through the English Channel, there would be a point about midway between Lowestoft and Brill on the coast of Holland, where no tide would be found, the two interfering waves exactly neutralizing each other. The accuracy of this prediction was established during a survey of that sea.

from geology,

The next instance we will take is a striking one as it shows how a clearly conceived hypothesis may be justifiably maintained even though results may be deduced from it which are at variance with present experience, if it can be shown that the hypothesis explains these exceptions. It is thus recorded by Herschel: "When Dr. Hutton expounded his "theory of the consolidation of rocks by the application of "heat, at a great depth below the bed of the ocean, and "especially that of marble by actual fusion; it was objected "that, whatever might be the case with others, with calcareous or marble rocks, at least, it was impossible to grant "such a cause of consolidation, since heat decomposes their "substance and converts it into quicklime, by driving off the "carbonic acid, and leaving a substance perfectly infusible,

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"and incapable even of agglutination by heat. To this he "replied, that the pressure under which the heat was applied "would prevent the escape of the carbonic acid; and that "being retained, it might be expected to give that fusibility "to the compound which the simple quicklime wanted. The "next generation saw this anticipation converted into an "observed fact, and verified by the direct experiments of Sir "James Hall, who actually succeeded in melting marble, by "retaining its carbonic acid under violent pressure" (*Nat. Phil.*, § 299).

and from electricity.

As a last example we will take one from the theory of Electricity, quoted by Jevons. "As soon as Wheatstone had "proved experimentallv that the conduction of electricity "occupies time, Faraday remarked in 1838, with wonderful "sagacity, that if the conducting wires were connected with "the coatings of a large Leyden jar, the rapidity of conduc- "tion would be lessened. This prediction remained unverified "for sixteen years, until the submarine cable was laid beneath "the Channel. A considerable retardation of the electric "spark was then detected, and Faraday at once pointed out "that the wire surrounded by water resembles a Leyden jar "on a large scale, so that each message sent through the "cable verified his remark of 1838" (*Prin. of Sci.*, p. 543).

(iv.) Crucial Instances.

A Crucial Instance or *experiment* decides between rival hypotheses.

It happens sometimes that two or even more different hypotheses will explain a great number of the phenomena in question. In such a case, it is necessary to look for some instance which can be explained on one only of these rival hypotheses. As Ueberweg puts it: "One single "circumstance, which admits of one explanation *only*, is more "decisive than a hundred others which agree in all points "with one's own hypothesis, but are equally well explained "on an opposite hypothesis" (*Logic*, Eng. Trans., p. 513). Such a test case is called a *Crucial Instance*, and if arrived at by experiment, the experiment is named *Experimentum Crucis*. The term is adopted from Bacon, who tells us that

it is "transferred from the crosses (or finger-posts) which "are put up in crossways to mark and point out different "ways" (*Nov. Org.*, Bk. II., Aph. 36). The essence of such an instance is that it should absolutely negative one hypothesis, and at the same time confirm another.

Examples deciding between the two theories of light.

The most remarkable instance in modern science in which many phenomena were equally well explainable on either of two incompatible hypotheses, was in the case of the long rivalry between the corpuscular and the undulatory hypotheses of light. "It is remarkable," says Jevons, "in how "plausible a manner both these theories agreed with the "ordinary laws of geometrical optics, relating to reflection "and refraction" (*Pr. of Sc.*, p. 520). But "if the undula-"tory theory be true, light must move more slowly in a dense "refracting medium than in a rarer one; but the Newtonian "theory assumed that the attraction of the dense medium "caused the particles of light to move more rapidly than in "the rare medium. On this point, then, there was com-"plete discrepancy between the theories, and observation "was required to show which theory was to be preferred. "Now by simply cutting a uniform plate of glass into two "pieces, and slightly inclining one piece so as to increase "the length of the path of a ray passing through it, experi-"menters were able to show that light does move more "slowly in glass than in air" (*op. cit.*, p. 521).

Another *experimentum crucis* in connexion with the same two hypotheses is related by Herschel. He says: "When "two very clean glasses are laid one on the other, if they be "not perfectly flat, but one or both in an almost impercep-"tible degree convex or prominent, beautiful and vivid "colours will be seen between them; and if these be viewed "through a red glass, their appearance will be that of alter-"nate dark and bright stripes. These stripes are formed "*between* the two surfaces in apparent contact, as anyone "may satisfy himself by using, instead of a flat *plate* of glass "for the upper one, a triangular-shaped piece, called a prism, "like a three-cornered stick, and looking through the inclined "side of it next the eye, by which arrangement the reflexion

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"of light from the upper surface is prevented from intermixing with that from the surfaces in contact. Now the "coloured stripes thus produced are explicable on both "theories . . . but there is a difference in one circumstance "according as one or the other theory is employed to explain "them. In the case of the Huyghenian [the undulatory] "doctrine, the intervals between the bright stripes ought to "appear *absolutely black;* in the other, *half bright*, when so "viewed through a prism. This curious case of difference "was tried as soon as the opposing consequences of the two "theories were noted by M. Fresnel, and the result is stated "by him to be decisive in favour of that theory which makes "light to consist in the vibrations of an elastic medium" (*Nat. Phil.*, § 218).

Example from atomic theory.

But it is not only between great rival theories that *experimenta crucis* may be called to decide. Whenever two explanations of a phenomenon are possible, such an experiment is required. For instance, in investigating the nature of the molecular structure of a gas: If the gas is compressed are the molecules themselves compressed, or are they only brought nearer to each other? Now, if the former is the case, the rate of vibration of the molecules will be altered. But a gas intercepts the colours whose rates of vibration correspond with those of its molecules. If, therefore, the rates of vibration are changed by compression of the gas, the gas will no longer intercept the same colours as before; but if the molecules are simply brought nearer together, the same colour waves will be intercepted, but to a greater extent. The latter being shown to be the case by experiment, it is proved that compression of the gas does not affect the size of the individual molecules of which it is composed, but simply brings them closer together.

152. Establishment of Hypotheses.

It remains to ask when a hypothesis may be regarded as fully established. The answer cannot be better given than in the words of Clifford: "In order to make out that your "supposition is true, it is necessary to show, not merely that

A hypothesis is established

"that particular supposition will explain the facts, but also "that no other one will" (*Lectures and Essays*, p. 137). In other words, it must be possible not only to reason from the hypothesis to the facts, but from the facts back to the hypothesis, without any other supposition being admissible at any step. The formal expression of this is that the hypothetical proposition, *If S is M, it is P*, which expresses the hypothesis must be reciprocal, so that *If S is P, it is M* is equally true. Whether this has been secured in any particular case is, of course, a material question, and resolves itself into the enquiry whether the conditions have been so thoroughly analysed that we are able to say that they, *as such*, produce the phenomena. Jevons, basing his theory of induction on the doctrine of chances, holds that no hypothesis can ever attain certainty, and that the only mode of establishing one as the most probable is by successively proving other suppositions false. But the mere disproof of one hypothesis does not aid in the establishment of another unless the instance which caused the former to be rejected also brought some positive evidence to the support of the latter; as was the case in each of the crucial experiments we considered in the last section. When a hypothesis is thus established as true it is called a *Theory*. Both words are often used loosely and regarded as interchangeable; but the best usage is to confine 'Theory' to a proved and established explanation.

when it is proved to be the only one which will explain the facts;

i.e. when it is expressed by a reciprocal hypothetical proposition.

A *Theory* is an established hypothesis.

One of the most important of these fully established theories is that of gravitation. Newton clearly proved by exact mathematical reasoning that no assumption except that of a force in the exact direction of the sun would so determine the course of each planet that the line from its centre to the centre of the sun should describe equal areas in equal times, as Kepler had demonstrated is the case in fact. Similarly, the formula that the force of gravity varies directly with the mass and inversely with the square of the distance was shown to be the only one whose consequences would agree with the phenomena.

Among such theories are gravitation,

The undulatory theory of light and the theory of the molecular constitution of matter are also regarded by such

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the undulatory theory of light and the theory of the molecular constitution of matter.

scientists as Clifford as thoroughly established. Experiments upon interference of light prove without any room for doubt that "in every ray of light there is some change or "other, whatever it is, which is periodic in time and in "space. By saying it is periodic in time, I mean that, at a "given point of the ray of light, this change increases up "to a certain instant, then decreases, then increases in the "opposite direction, and then decreases again, and so on "alternately. . . . By saying that this phenomenon is "periodic in space, I mean that, if at any given instant you "could examine the ray of light, you would find that some "change or disturbance, whatever it is, has taken place all "along it in different degrees. It vanishes at certain points, "and between these it increases gradually to a maximum "on one side and the other alternately. . . . Now this fact, "which is established by experiment, and which is not a "guess at all—the fact that light is a phenomenon periodic "in time and space—is what we call the wave theory of "light. . . . But we can see more than this . . . We know "that a ray of light or heat is capable of doing work . . . "therefore this periodic phenomenon which takes place in "the ray of light is something or other which possesses "mechanical energy, which is capable of doing work. We "may make it, if you like, a mere matter of definition, and "say: Any change which possesses energy is a motion of "matter. . . . In that sense, and in that sense only, it is a "matter of demonstration, and not a matter of guess, that "light consists of the periodic motion of matter, of some-"thing which is between the luminous object and our eyes. "But that something is not matter in the ordinary sense of "the term; it is not made up of such molecules as gases "and liquids and solids are made up of. This last state-"ment again is no guess, but a proved fact. . . . In order "that separate molecules may carry about a disturbance, "it is necessary that they should travel at least as fast as "the disturbance travels. Now we know . . . that the "molecules of gas travel at a very ordinary rate—about "twenty times as fast as a good train. But, on the con-

"trary, we know by the most certain of all evidence, by "five or six different means, that the velocity of light is "200,000 miles a second. By that very simple consideration "we are able to tell that it is quite impossible for light to "be carried by the molecules of ordinary matter, and that "it wants something else that lies between those molecules "to carry the light" (Clifford, *op. cit.*, pp. 138-140). Thus the theory is proved to be true as it can be argued back to from the facts, and at no step is a different supposition possible. Further, the absorption of different coloured rays of light—which are known to depend on the rate of vibration—by different gases, proves that the gases possess corresponding rates of vibration. The further fact that compression of a gas does not affect this rate of vibration—as is proved by the same coloured rays being still absorbed—proves that the vibration does not belong to the gas as a whole but to the separate parts of it. "Now, by such "reasoning as this," says Clifford, "it seems to me that the "modern theory of the constitution of matter is put upon "a basis which is absolutely independent of hypothesis" (*ibid.*, pp. 140-2).

Simplicity is not a test of the truth of a hypothesis,

It is sometimes said that the simplicity of a hypothesis is a test of its truth. But, in the ordinary meaning of 'simplicity' this cannot be accepted. Nature is complex, and its complexity cannot be represented by simple theories. As Herschel reminds us: "When we examine those instances "of Nature's workmanship which we can take to pieces "and understand, we find them in the highest degree arti-"ficial in our own sense of the word" (*Nat. Phil.*, § 215). No doubt, if two hypotheses are presented, one of which explains the facts in a simpler way than the other—as, *e.g.*, by the assumption of the action of a known instead of an unknown agent—it is *prima facie* to be preferred for investigation; but the test of its acceptance must be that thorough-going and reciprocal relation to the observed facts which we have just considered. It is only when the hypothesis deals with "those fundamental forces, whose constant "action under all sorts of secondary conditions makes up the

except in the sense of more thorough systematization.

"complex tissue of physical processes" that simplicity is a characteristic of its truth; and it is so then "because for "those pure cases only one of the simpler forms of regular "coherence . . . between cause and effect is in fact *con-"ceivable*" (Lotze, *op. cit.*, vol. ii., p. 89). This leads us to one sense of the word in which true hypotheses are marked by simplicity, in the sense, namely, that a true theory is found to explain wider and wider ranges of reality, and thus to make our concept of the universe a simpler, because a more systematic, one. This is the sense in which Whewell regards simplicity as a test of a true hypothesis. With true theories he tells us "all the additional suppositions *tend to simplicity* "and harmony; the new suppositions resolve themselves "into the old ones, or at least require only some easy modi-"fication of the hypothesis first assumed: the system "becomes more coherent as it is further extended. The "elements which we require for explaining a new class of "facts are already contained in our system. Different "members of the theory run together, and we have thus a "constant convergence to unity. In false theories, the con-"trary is the case. The new suppositions are something "altogether additional;—not suggested by the original "scheme; perhaps difficult to reconcile with it. Every such "addition adds to the complexity of the hypothetical system, "which at last becomes unmanageable, and is compelled to "surrender its place to some simpler explanation" (*Nov. Org. Ren.*, p. 91).

CHAPTER V.

ANALYSIS OF THE GIVEN.

153. Observation and Experiment.

(i.) Nature of Observation.

Theories must be established by analysis of phenomena.

We must now consider the means by which the truth of theories about reality can be established. As Mill says: "The order of nature, as perceived at a first glance, presents "at every instant a chaos followed by another chaos" (III., vii., § 1); it is only the demand of the rational mind for system that compels us to regard this chaos as only apparent, and to seek to find beneath it regularity and law. And the only method by which we can hope to accomplish this is by analysing the complex phenomena presented to us, and examining them in detail. This analysis is both mental and practical. It is mental first, in that before we attempt to make an actual examination of the real we must decide as to the character of that examination. But it is also practical, or we have mere theorizing out of all relation to fact. Nor do we ever doubt the theoretical possibility of such practical analysis; when we actually fail in any attempt to make it, we attribute the failure to the weakness of our perceptive powers, and not to the character of the phenomena of experience.

This analysis must be practical as well as mental.

All analysis implies isolation and selection.

Now all analysis of phenomena implies isolation, and all isolation is more or less artificial. Attention is concentrated on some elements of the complex totality of presentation, and is withdrawn from all others. It is so, even in the simplest act of perception, which is always selective. It is not the whole complex of sense impressions we attend to at

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any moment, but only those in which, for some reason or other, we are interested. And the character of the selection made is determined, not by the resemblance of sensuous impressions to each other, but by the necessity of explaining their connexion in experience. Thus we attribute one group of impressions received through various senses to one object, another group to another object, others to the simultaneous or successive connexions of those objects. Even in the simplest case, that is to say, we interpret our experience under the forms of identity, time, and space. From the first, then, we have in observation the selective and constitutive activity of mind in determining what is a thing or an event, and this activity can only be exercised under the guidance of previous knowledge. Moreover, the understanding of a fact is also determined by what the observer already knows; a piece of machinery, for instance, means more to an engineer than to an observer with equally good sense organs who is ignorant of machinery; the former, indeed, actually *sees* more in it than does the latter. And the same holds with the most elaborate observations. The scientific observer selects the phenomena he will examine, and leaves out of consideration the far greater mass of phenomena which he considers immaterial to the purpose he has in hand. And both the intelligence with which the phenomena to be observed are selected, and the power of finding meaning in the observations when they are made, depend upon the character of the mind of the observer—not only upon his previous knowledge, but also upon that indescribable insight which marks the true discoverer. These considerations show how false is the empiricist doctrine that all knowledge is derived from mere sensuous impressions. What one finds in any object examined is largely determined by what one brings to the examination. For instance, many broken crystals had been seen without the fracture suggesting the laws of crystalline formation. It was only when the phenomenon was observed by a well-prepared mind, such as was that of Haüy, that the observation bore fruit [*cf.* § 147 (ii.) (*c*)]. Professor Mackenzie well expresses this when he

Both observation and interpretation imply previous knowledge, and therefore involve inference.

says: "The rôle of the mere observer must always be a humble "one, even in the case of those sciences which offer him the "most abundant scope. The true 'seer,' indeed, is the rarest "of all discoverers: but the true seer is one who brings to his "observation more than he finds in it. The drudgery of the "patient interrogator of nature is made divine only when it "is inspired by ideas which are not objects of observation" (*Introd. to Social Philosophy*, p. 13). To put it briefly; all perception involves selection and interpretation, and both these are inference. In simple cases, no doubt, the inference is subconscious; but the total process does not differ in essence from that in which the inferential aspect is most prominent.

Observation may be vitiated by bias.

To this inferential character of observation is, however, due one of its chief difficulties—the difficulty of distinguishing between what is really perceived and what is inferred. We cannot separate our ideas from the material to which they give meaning, and hence comes the danger that the facts may be wrongly interpreted through some previous bias. We are all apt to find in the facts of observation what we expect or wish to find in them. As Jevons says: "It is difficult to find persons who can with perfect fairness "register facts for and against their own peculiar views" (*Prin. of Sci.*, p. 402). Logic can give no rules for the avoidance of this difficulty. Indeed, as the whole matter is psychological and subjective, it does not really enter into the province of logic at all; the means of training the power of accurate observation belong to the general theory of education, not to logic. It is sufficient to point out the danger, and to indicate the qualities of mind by which it can be avoided. These are neatly summed up by Jevons: "The "successful investigator," he says, "must combine diverse "qualities; he must have clear notions of the result he "expects and confidence in the truth of his theories, and "yet he must have that candour and flexibility of mind "which enable him to accept unfavourable results and "abandon mistaken views" (*op. cit.*, p. 404).

Nor is the matter of selection less difficult. The

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Selection requires abundant knowledge.

phenomena it is desired to investigate are arbitrarily isolated, and this isolation may not be as complete as it appears to be. Conditions of which no account has been taken may really be operative, and interfere with the course of events it is desired to study. For the avoidance of this difficulty also, logic can give no exact rules. It is all-important to select for examination just those conditions which are essential, and to take account of all the conditions which are operative in the phenomena under investigation. But mere rules will not enable an observer to do this. The extent to which he can do it depends upon the thoroughness with which he can understand the phenomena, and this is largely determined by the knowledge he brings to the examination. Thus, as Herschel says: "To make a perfect "observer . . . an extensive acquaintance is requisite, not "only with the particular science to which his observations "relate, but with every branch of knowledge which may "enable him to appreciate and neutralize the effect of "extraneous disturbing causes" (*Discourse on Nat. Phil.*, § 127).

Non-observation does not prove non-existence of a phenomenon, unless its existence would certainly involve its observation.

One other danger of observation must be noticed—the tendency to infer from the non-observation of a phenomenon that it is non-existent. To what extent such an inference is justifiable depends upon the improbability that the phenomenon would have escaped observation had it existed. As Jevons says, "The earth's surface has been sufficiently "searched to render it highly improbable that any terrestrial "animals of the size of a camel remain to be discovered" (*op. cit.*, p. 412). But the smaller the phenomenon the more likely it is to escape observation. A good instance of this is the length of time during which the existence of the new constituent of the atmosphere, argon, was unsuspected. When a proposition is accepted on merely negative evidence, care should be taken to make that evidence as complete as possible. An excellent example of this care is seen in the investigations which led Darwin, on purely negative evidence, to conclude that certain orchids secrete no nectar. Many observations both of German naturalists and of Darwin

Negative evidence should be made as complete as possible.

himself seemed to point to this conclusion, but, says Darwin: "Notwithstanding these several facts I still suspected that "nectar must be secreted by our common Orchids, and I "determined to examine *O. morio* rigorously. As soon as "many flowers were open, I began to examine them for "twenty-three consecutive days: I looked at them after "hot sunshine, after rain, and at all hours: I kept the "spikes in water, and examined them at midnight, and early "the next morning: I irritated the nectaries with a bristle, "and exposed them to irritating vapours: I took flowers "which had lately had their pollinia removed by insects, of "which fact I had independent proof on one occasion by "finding grains of some foreign pollen within the nectary; "and I took other flowers, which judging from their position "on the spike, would soon have had their pollinia removed; "but the nectary was invariably quite dry. . . . I one day "saw various kinds of bees visiting repeatedly the flowers "of this same Orchid, so that this was evidently the proper "time to examine their nectaries; but I failed to detect "under the microscope even the minutest drop of nectar. "So it was with the nectaries of *O. maculata* at a time when "I repeatedly saw flies of the genus Empis keeping their "proboscides inserted into them for a considerable length of "time. *Orchis pyramidalis* was examined with equal care "with the same result, for the glittering points within the "nectary were absolutely dry. We may therefore safely "conclude that the nectaries of the above-named Orchids "neither in this country nor in Germany ever contain "nectar" (*Fertilization of Orchids*, pp. 38-9).

Scientific instruments embody knowledge.

If simple observation is largely dependent upon previous knowledge still more so is this the case with observations made with the aid of scientific instruments, for all such instruments embody in themselves much knowledge. Only a very skilled observer is able to use many of the most accurate instruments, because he alone can turn to practical account the knowledge they embody, and can detect and allow for errors incidental to their use. Thus, not only the matter which is observed, but the accuracy of the observa-

tion depends on previous knowledge; and such accuracy is always a matter of inference from the application of such knowledge. Such instruments as microscopes, telescopes, balances, and other contrivances for measurement are, therefore, not merely, or even chiefly, aids to sense perception. As Dr. Bosanquet says: "Science does not rest on abnormal "acuteness of perception, but on inferences drawn from "perfectly normal perception. The power of vision for "science is not in the least proportioned to its actual im-"mediate penetration; the structure of inference into which "it enters is the main thing, and the acuteness of the "observer's eye, though useful, is a subordinate element" (*Knowledge and Reality*, p. 329).

(ii.) Relation of Observation and Experiment.

The end aimed at in observation is full and exact knowledge of all operative conditions,

All observation, we have just seen, implies isolation—mental if not physical—of the phenomena examined. This isolation is perfect, and permits of definite and exact observation only when all the material conditions of the phenomenon under consideration are fully and exactly known. But this is very frequently not the case. Nature presents us with complex totalities in which the pure connexion we are seeking is obscured by the many extraneous elements with which it is mixed up. And further, natural processes are, in some cases, so extremely slow and gentle that they escape observation. As Lavoisier remarked, the decomposition of water had been continually going on, although it had never been observed before his time. It is evident, therefore, that it is frequently necessary for the observer himself to determine the conditions under which he will examine the phenomenon. As Jevons says: "To observe with accuracy "and convenience we must have agents under our control, "so as to raise or lower their intensity, to stop or set them "in action at will" (*Princ. of Sci.*, p. 401). Such a definitely determined observation is called an *Experiment*. It is evident, then, that the difference between observation and experiment is one, not of kind, but of degree. Experiment is not a distinct method of acquiring knowledge, but is rather the

and this can frequently be attained only in experiment.

Experiment is observation under conditions determined by the observer.

preparation of the phenomena under consideration so that the observation may be made under known, instead of under unknown, conditions, and shall, consequently, attain the highest possible degree of accuracy. The object of experiment is to eliminate unessential elements in the phenomena, so that the pure connexion sought for may be made manifest. Experiment is, thus, more abstract than observation, in that the process of selection and isolation is carried further. When complete knowledge of the essential conditions can be obtained by observation, experiment is unnecessary. Thus, "experiment only has an advantage over observation, "in so far as it is capable of supplementing the usual "deficiencies of the latter; its function is to furnish us "with suitable and fruitful observations instead of the un-"suitable and unfruitful ones which offer themselves" (Lotze, *Logic*, Eng. Trans., vol. ii., p. 40). It is evident, however, that this function is a most important one, and that by experiment knowledge can be advanced much more rapidly and surely than would be possible were we confined to simple observation; for, in the latter case the chief difficulty is to find out what conditions are operative, and this often cannot be done with precision, whilst it is the very point which the experimenter determines for himself. Moreover, in observation one has to wait till suitable instances can be found, and this involves much delay even when such instances are ultimately forthcoming, which they very often are not. It is, generally, only by experimenting that instances sufficiently varied in their points of identity and difference can be obtained to enable us to ascertain what conditions are essential. On the other hand, as Lotze points out, "observation often acquaints us with broad character-"istics of phenomena, which in experiment would have been "obscured by special conditions" (*ibid.*, p. 40). Again, some natural processes are so slow that experiment is impossible. The geologist is, for example, confined to observation as to the structure and formation of rocks and the occurrence of fossils; and the biologist is similarly unable to experiment on the evolution of species.

Its object is to eliminate unessential elements.

Experiment is essential to advance of exact knowledge,

but simple observation is also necessary.

In experiment the conditions determined are an integral part of the phenomenon observed.

The use of a scientific instrument does not make an observation into an experiment, unless the instrument modifies the object which is being observed. Thus we invariably speak of *observing* with a telescope or a microscope. The distinction is very clearly put by Dr. Bosanquet: "The fact is then that experiment is not "merely observation under artificial or determinate con-"ditions, but *observation under determinate conditions which* "*constitute an integral part of the image or product to be* "*observed.* Thus, common dissection is not experiment, "though it introduces conditions in the way of separation "and demarcation as definite as anything can be; but vivi-"section is experiment, because the determinate conditions "it produces enter as factors into the action of the organism "observed" (*Logic*, vol. ii., p. 145).

'Natural experiments' are intermediate between simple observation and pure experiment.

The transition, then, between observation and experiment is a gradual one, as might be expected from the fact that they differ only in degree. This transition appears in another aspect in what Jevons calls "natural experiments," and which he thus describes: "It may readily be seen that we "pass upwards by insensible gradations from pure observation "to determinate experiment. When the earliest astronomers "simply noticed the ordinary motions of the sun, moon, "and planets upon the face of the starry heavens, they "were pure observers. But astronomers now select precise "times and places for important observations of stellar "parallax, or the transits of planets. They make the earth's "orbit the basis of a well arranged *natural experiment*, as it "were, and take well considered advantage of motions which "they cannot control. Meteorology might seem to be a "science of pure observation, because we cannot possibly "govern the changes of weather which we record. Never-"theless we may ascend mountains or rise in balloons, like "Guy-Lussac and Glaisher, and may thus so vary the points "of observation as to render our procedure experimental. "We are wholly unable either to produce or prevent earth-"currents of electricity, but when we construct long lines "of telegraph, we gather such strong currents during periods

"of disturbance as to render them capable of easy observa-"tion" (*Prin. of Sci.*, pp. 400-1).

(iii.) Function of Experiment.*

Experiment is always guided by hypothesis.

Appeal to experiment is, then, necessary whenever simple observation alone will not make plain all the essential conditions of a phenomenon; and its object is to eliminate all conditions which are not specially operative in the particular case under consideration. Did we know all the conditions present, and needed but to decide which were operative and which were not, it would appear to be theoretically possible to empirically determine this question by trying every possible combination of both the presence and the absence of these conditions. Practically the enormous number of experiments involved would render this impossible; and the fact that conditions are not independent elements of reality would add another difficulty. For the removal of one condition not infrequently affects the action of those left behind, and similarly the addition of a new condition may cause an alteration in the result which could not be produced by this condition by itself but only through its union with others. However, as we have seen, experiment is never carried on in such a merely empirical manner. It is always guided by hypothesis suggested by analogy, after some examination of the given phenomena.

The goal of experiment is to establish a reciprocal hypothetical proposition.

We may, then, express symbolically the problem to be solved: Analogy has given us the suggestion that S may be P because both share in the common nature $x\ y\ z\ R$, where x, y, z represent definite known elements, and R the unanalysed, and probably partially unknown, residual nature of the concrete phenomenon under examination. The function of experiment is to analyse S into $a\ b\ c\ R$, so that to each element x we can assign its invariable condition a, all other elements, of course, being for this purpose, included in R. A similar analysis of P is not necessary, as the analysis of S will make clear what is the connexion between P and $x\ y\ z\ R$.

* This discussion is broadly based on the treatment of the subject by Dr. Bosanquet (*Logic*, Bk. II., Ch. iv.).

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and whether, therefore, that connexion is one which is the ground of an essential connexion between *S* and *P*. But to establish *a* as the invariable and necessary condition of *x*, it is necessary to prove not merely the pure hypothetical *If S is a then it is x*, but also its reciprocal *If S is x then it is a.* The goal of experiment is, then, to establish one or more of such reciprocal hypothetical judgments, each expressing a necessary connexion of elements, and each, of course, highly abstract, as it deals with only one relation in the complex totality of which every concrete phenomenon is composed. The only sure way of establishing the reciprocal judgment *If S is x then it is a* is to examine instances of the absence of *a* ($\bar{a}$), and so try to show that when *a* is absent *x* is also absent ($\bar{x}$); in other words to establish the judgment *If S is* $\bar{a}$ *then it is* $\bar{x}$, which is the obverted contrapositive of, and, therefore, equivalent to, *If S is x then it is a* (*see* § 105[1]). Hence, in every conclusive experiment there is comparison of the phenomenon both in the presence and in the absence of that particular condition we are investigating.

The reciprocal judgment must be established by examination of negative instances.

This latter—which is what chemists sometimes call a *Blind Experiment*—is absolutely essential to the establishment of the reciprocal judgment. But it is more difficult than the positive experiment; for sometimes the effect may really be present but be so small that it either escapes notice altogether or is included in and confounded with a larger effect. A striking instance of this is the fact, already referred to, that though many experiments had been worked on the constitution of the atmosphere, yet argon so long escaped notice. Jevons quotes a very instructive example of the difficulties of negative experiments. He says: "A curious "instance of false negative inference is furnished by experi-"ments on light. Euler rejected the corpuscular theory on "the ground that particles of matter moving with the "immense velocity of light would possess momentum, of "which there was no evidence. Bennet had attempted to "detect the momentum of light by concentrating the rays of "the sun upon a delicately balanced body. Observing no

Negative experiment is more difficult than positive.

[1] First Edition, § 119.

"result, it was considered to be proved that light had no "momentum. Mr. Crookes, however, having suspended thin "vanes, blacked on one side, in a nearly vacuous globe, found "that they move under the influence of light. It is now "allowed that this effect can be explained in accordance "with the undulatory theory of light, and the molecular "theory of gases. It comes to this—that Bennet failed to "detect an effect which he might have detected with a better "method of experimenting; but if he had found it, the "phenomenon would have confirmed, not the corpuscular "theory of light, as was expected, but the rival undulatory "theory. The conclusion drawn from Bennet's experiment "was falsely drawn, but it was nevertheless true in matter" (*Prin. of Sci.*, p. 435). Again, as was pointed out in (ii.) above, some conditions—*e.g.* gravity—cannot be removed, and in experimenting on phenomena in which such permanent conditions are essential, the experimenter is reduced to varying their intensity as far as possible.

It is necessary to exclude all inoperative elements and them only.

To render a negative experiment conclusive is, therefore, difficult, though it is, at the same time, essential. Such a conclusion can be justified only on the assumption that our analysis of the phenomenon is ultimate. And this leads us to an enquiry into what is meant by R in our symbolism $x\ y\ z\ R$. As the universe is a systematic whole, R is, in its primary meaning, that whole system. In this sense an ultimate analysis is obviously impossible. We, therefore, limit R in every case by the elimination of what we have reason to think is inoperative so far as the special character of the relation we are considering—*If S is a then it is x*—is concerned. And it is obvious, as was pointed out above, [*see* (i.)] that this limitation can only be made with any probability of justification by one who has large and varied, as well as systematic, knowledge both of the subject under investigation and of kindred subjects. There is a limitation then of R, first, to those conditions which contain the ground of both a and $\bar{a}$; this we may symbolize as R_2, whilst we express the whole unanalysed totality which we put on one side as R_1. And, secondly, we require to further select out

This limitation implies extensive knowledge,

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of R_2—which being the ground of both a and $\bar{a}$ is indifferent as to the distinction between them—R_3 which is the sufficient ground of the positive phenomenon x and which is, consequently, ultimately reducible to a. Now, the exclusion of R_1 from consideration, can only be justified by showing that it is really irrelevant, and this can only be done by finding a number of instances in which R_2 (including R_3) is sufficient for the presentation of a and $\bar{a}$ as required. Number of instances is here valuable as confirming the hypothesis that R_2 is the sufficient ground of the phenomena in question.

and is liable to error.

Of course, this limitation is liable to error when the phenomena are complex, and such error can only be detected by extremely careful and varied experiments to determine whether any condition is operative which had not been suspected, and had, therefore, been relegated to the unanalysed R_1. Jevons quotes a striking example: "In more "than one case the unsuspected presence of common salt "in the air has caused great trouble. In the early experi-"ments on electrolysis it was found that when water was "decomposed, an acid and an alkali were produced at the "poles, together with oxygen and hydrogen. In the absence "of any other explanation, some chemists rushed to the "conclusion that electricity must have the power of "*generating* acids and alkalies, and one chemist thought he "had discovered a new substance called *electric acid.* But "Davy proceeded to a systematic investigation of the cir-"cumstances, by varying the conditions. Changing the "glass vessel for one of agate or gold, he found that far "less alkali was produced; excluding impurities by the use "of carefully distilled water, he found that the quantities "of acid and alkali were still further diminished; and "having thus obtained a clue to the cause, he completed "the exclusion of impurities by avoiding contact with his "fingers, and by placing the apparatus under an exhausted "receiver, no acid or alkali being then detected. It would "be difficult to meet with a more elegant case of the detec-"tion of a condition previously unsuspected" (*Prin. of Sci.*, pp. 428-9).

154. Method of Qualitative Analysis.

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(i.) Statement of Method.

We must now enquire into the logical character of the methods by which the investigator succeeds in establishing laws of nature. Such enquiry is concerned with concrete reality, though not with mere instances. The number of instances consulted is logically a matter of indifference, and depends on the nature of the phenomena investigated, and the greater or less difficulty of excluding from consideration all immaterial elements. The logical aim is to reach the pure connexion between the conditions and their consequence unimpeded by any irrelevant details. This we expressed symbolically in the last sub-section. But all symbolic expression suggests that the matter is a much simpler one, and one that can be made much more mechanical, than is really the case. "Mechanical Induction," as Dr. Bosanquet says, "is an idle dream" (*op. cit.*, ii., p. 135). In every observation an immense residue of phenomena, which we have symbolized by R, is left out of account, and presumed to be indifferent. Now much of this R is, in every case, incapable of actual removal ; the aim is then to secure that it includes nothing which is not known to be present, and whose influence, if it existed, could be determined and allowed for. The exclusion of R is, therefore, logically justified only so far as R has been analysed. In other words, to establish a universal proposition as true it is necessary to secure that conditions are fully known. Every such proposition rests on a connexion which exists in reality, and which is suggested by analogy. Now, in our examination of analogy, it was seen that the conclusion may be confirmed by negative instances, and that as this confirmation advances in completeness the inference passes beyond the bounds of analogy [*see* § 149 (iv.)]. It is the completion of this negative inference which is the characteristic and essential feature in that inductive analysis of phenomena with which we are now concerned. The logical universal must be limited throughout by negations which mark the exact boundaries

Number of instances is not logically important.

Induction cannot be made mechanical.

Every universal must be limited by negations,

of the content with which it deals. It is only thus that the scope of the application of the universal—*If S is a it is x*—can become manifest. This process of assigning limits to the universal will bring into view any real or apparent exceptions. Each of these must be carefully considered. If it is really an exception, the suggested universal will not hold, at any rate in the form in which it was first suggested. But, if further examination shows that the exception is only apparent, and due to some misinterpretation of the facts, then "the exception proves the rule"—or helps to prove it—in the only logical sense of the words. The logical universal, then, "must be purged by exceptions and finally "limited by negations" (Bosanquet, *Logic*, vol. ii., p. 117). And this limitation must be thorough and systematic, so that, throughout, every suggested positive connexion *If S is a it is x* is both defined and corroborated by the negative connexion *If S is* $\bar{a}$ *it is* $\bar{x}$.

and so moulded as to remove all exceptions.

In all investigation only one change should be made at a time.

In investigating any suggested connexion, it is essential that only one variation should be introduced at a time. Otherwise the resulting change could not be definitely assigned to its conditions, as room would be left for doubt as to which of these new variations, or which of their possible combinations, conditioned it. Similarly, in examining negative instances, only the one agent whose influence is suggested should be excluded, as otherwise the absence of the result cannot be definitely assigned to the removal of the agent in question, for the possibility is left that it may be due to the withdrawal of the other possible agents which were excluded with it. "The great method of experiment," says Jevons, "consists in removing, one at a time, each of those conditions "which may be imagined to have an influence on the result" (*op. cit.*, p. 417). The same principle underlies Mill's 'Method of Difference' though it is there but imperfectly applied (*see* § 155).

(ii.) Examples of Method.

Examples of perceptive analysis :—

The method of analysis by consideration of exceptions, and consequent moulding of the universal rule, and by its limita-

tion by negative instances will be best apprehended by a consideration of its application in the establishment of several scientific truths.

Establishment by observation of the hypothesis that flowers are beautiful in order to attract insects.

(*a*) *Origin of Beauty of Flowers.* We will first take an instance in which experiment could not be resorted to, and which is in a transitional stage between analysis by analogy and the more perfect analysis which experiment makes possible. The hypothesis that flowers "have been rendered "conspicuous in contrast with the green leaves, and in conse- "quence at the same time beautiful, so that they may be "easily observed by insects" was advanced by Darwin in his *Origin of Species* (p. 161). He thus supports it: "I have "come to this conclusion from finding it an invariable rule "that when a flower is fertilized by the wind it never has a "gaily-coloured corolla. Several plants habitually produce "two kinds of flowers; one kind open and coloured so as to "attract insects; the other closed, not coloured, destitute of "nectar, and never visited by insects" (*ibid.*). To this Dr. Wallace adds: "The argument in favour of this view is now "much stronger than when he [Darwin] wrote; for not only "have we reason to believe that most of these wind-fertilized "flowers are degraded forms of flowers which have once "been insect-fertilized, but we have abundant evidence that "whenever insect-agency becomes comparatively ineffective, "the colours of the flowers become less bright, their size and "beauty diminish, till they are reduced to such small, "greenish, inconspicuous flowers as those of the rupture-wort ". . . the knot-grass . . . or the cleistogamic flowers of the "violet. There is good reason to believe, therefore, not only "that flowers have been developed in order to attract insects "to aid in their fertilization, but that, having been once "produced, in however great profusion, if the insect races "were all to become extinct, flowers (in the temperate zones "at all events) would soon dwindle away, and that ultimately "all floral beauty would vanish from the earth" (*Darwinism*, p. 332). Here we see the attempt to establish the reciprocal proposition: 'If flowers are insect-fertilized, they are beautiful' by an appeal to negative instances (flowers not fertilized

by insect agency) as well as to positive instances, and to do this, moreover, not only generally but in gradation, connecting the amount of insect fertilization with the degree of beauty, *i.e.* for every particular value of *a* in the general proposition *If S is A it is X*, trying to establish a definite value of *x*. From the nature of the case, however, this cannot be done perfectly, *i.e.* with quantitative exactness.

Establishment, chiefly by observation, of the theory that vegetable mould is due to the action of earth-worms.

(1) Positive Instances.

(*b*) *Formation of Vegetable Mould.* As a second example, in which the analysis is more thorough, and the conclusion more definite as well as more certain, we will take Darwin's investigations into the formation of vegetable mould through the action of earth-worms. Here also the analysis was conducted nearly entirely by simple observation. If vegetable mould is due to the action of earth-worms, that is, if earth "is "brought up by worms from beneath the surface, and is "afterwards spread out more or less completely by the rain "and wind," then small objects left on the surface will gradually become buried. Mr. Darwin records many observations to establish this proposition. For instance: "In the "spring of 1835, a field, which had long existed as poor "pasture, and was so swampy that it trembled slightly when "stamped on, was thickly covered with red sand so that the "whole surface appeared at first bright red. When holes were "dug in this field after an interval of about two and a half "years, the sand formed a layer at a depth of $\frac{3}{4}$ in. beneath the "surface. In 1842 (*i.e.* seven years after the sand had been laid "on) fresh holes were dug, and now the red sand formed a "distinct layer, 2 inches beneath the surface. . . . Imme- "diately beneath the layer of red sand, the original substra- "tum of black sandy peat extended" (Darwin, *Vegetable Mould and Earth Worms*, pp. 134-5). But may the character of the land be disregarded, or is that an essential element? This point was settled by observing that in cases of land differing widely from that on which the first observations were made, whenever worms were present the result was the same. To take an unpromising instance: Darwin records that "The chalk formation extends all round my house in "Kent; and its surface, from having been exposed during an

"immense period to the dissolving action of rain-water, is "extremely irregular, being abruptly festooned and pene- "trated by many deep well-like cavities. During the dissolu- "tion of the chalk, the insoluble matter, including a vast "number of unrolled flints of all sizes, has been left on the "surface, and forms a bed of stiff red clay, full of flints, and "generally from 6 to 14 feet in thickness. Over the red "clay, wherever the land has long remained as pasture, there "is a layer a few inches in thickness, of dark-coloured "vegetable mould" (*ibid.*, pp. 137-9). In some instances, the observations partook of the nature of experiment, as when chalk was spread on a pasture "for the sake of observing at "some future period to what depth it would become buried," and it was found that after twenty-nine years, the chalk was 7 inches below the surface. Even a steeply sloping field "thickly covered with small and large flints," was after thirty years so completely covered that "a horse could gallop over "the compact turf from one end of the field to the other, "and not strike a single stone with his shoes. . . . This was "certainly the work of the worms, for though castings were "not frequent for several years, yet some were thrown up "month after month, and these gradually increased in "numbers as the pasture improved" (*ibid.*, pp. 143-4). Moreover, "the specific gravity of the objects does not affect "their rate of sinking, as could be seen by porous cinders, "burnt marl, chalk and quartz pebbles, having all sunk to "the same depth within the same time" (*ibid.*, p. 157). We have, necessarily, only cited a very few of the observations made, but the total evidence goes to support a connexion between the existence of earth-worms and the formation of mould.

(2) Exceptions shown to be only apparent.

However, there are apparent exceptions—large boulders do not sink. All the hollow spaces between such a boulder and the earth will be filled up, and the surface of the ground will be raised to a height of several inches all round the edge of the stone. But further examination shows that the exceptions are only apparent, and really prove the rule. "If . . . a "boulder is of such huge dimensions, that the earth beneath

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"is kept dry, such earth will not be inhabited by worms, and "the boulder will not sink into the ground" (*ibid.*, p. 149). In one case in which a large stone "67 inches in length, "39 in breadth, and 15 in thickness" . . . had only "sunk "about two inches into the ground" in thirty-five years, "on "digging a large hole to a depth of 18 inches where the stone "had lain, only two worms and a few burrows were seen, "although the soil was damp and seemed favourable for "worms. There were some large colonies of ants beneath "the stone, and possibly since their establishment the worms "had decreased in number" (*ibid.*, pp. 152-3).

(3) Negative Instances.

Thus, the apparent exceptions turn out upon closer examination to be negative instances—where there are no worms, there is no sinking of objects. A negative instance on a larger scale is recorded. "I examined in Knole Park a "dense forest of lofty beech-trees beneath which nothing "grew. Here the ground was thickly strewed with large "naked stones, and worm-castings were almost wholly absent. "Obscure lines and irregularities on the surface indicated "that the land had been cultivated some centuries ago. It "is probable that a thick wood of young beech-trees sprung "up so quickly, that time enough was not allowed for worms "to cover up the stones with their castings, before the site "became unfitted for their existence" (*ibid.*, pp. 144-5). Both positive and negative instances, then, go to support the universal propositions "If there are worms, there is vegetable "mould" and "If there is vegetable mould, there are "worms."

(4) Objections as to inadequacy of the agency refuted by quantitative investigations.

But certain objections must be met as to the inadequacy of the suggested agency to produce the results. Investigations were, therefore, made as to the number of worms existing in a measured space. Hensen, from counting those found in a piece of garden, calculated the number at 53,767 in an acre, though he "believes that worms are here twice as "numerous as in cornfields" (*ibid.*, pp. 158-9). But more definite results were obtained in estimating the weight of the castings. The results of four carefully examined cases give that in a year castings calculated to yield from seven and a

half to over eighteen tons per acre are ejected (*cf. ibid.*, pp. 168-9). By carefully breaking up the dried castings, and pressing them down in a measure, the cubical content was found, and allowance being made for the lesser degree of compactness of the triturated castings as compared with mould, it was calculated that the above mentioned results would yield a layer of mould from an inch to an inch and a half in thickness in ten years. This was found to be rather less than the observed depth to which objects had sunk in that time. But allowance must be made for "the loss which "the weighed castings had previously undergone through "being washed by rain, by the adhesion of particles to the "blades of the surrounding grass, and by their crumbling "when dry," and also for the lesser agency of burrowing larvæ and insects, especially ants, and of moles. "But," says Darwin, "in our county these latter several agencies "appear to be of quite subordinate importance in comparison "with the action of worms" (*ibid.*, pp. 172-3).

The analysis of such an instance as the above shows how complex and difficult the whole process is, and also exemplifies how even such an investigation, conducted as it was practically by observation as distinguished from experiment, has to appeal to quantitative considerations, before exact results can be definitely established.

Investigation as to whether ants can communicate by sound.

(*c*) *The Sense of Hearing in Ants.* In the next case which we take for examination, though observation is still the prevailing mode of investigation, yet there is experiment of a simple kind designed to secure a definite limitation of conditions. The example is drawn from Sir John Lubbock's book on *Ants, Bees, and Wasps.* The question to be decided is whether ants can communicate with each other through the sense of hearing, or whether they are deaf and dumb. Analogy of structure would suggest that they can hear, for they have antennæ, and, says Sir John, "I have myself made "experiments on grasshoppers, which convinced me that "their antennæ serve as organs of hearing" (p. 221). But, if ants can hear, they can communicate by sound; and, on the contrary, if they are deaf, sounds will not affect them,

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and if sounds do not affect them, they may be presumed to be deaf. On this latter point Sir John Lubbock says: "I "have never succeeded in satisfying myself that my ants, "bees, or wasps heard any of the sounds with which I tried "them. I have over and over again tested them with the "loudest and shrillest noises I could make, using a penny "pipe, a dog-whistle, a violin, as well as the most piercing and "startling sounds I could produce with my own voice, but all "without effect" (*ibid.*, pp. 221-2). The ants were tested by a variety of noises being made as near them as possible while they were passing to and fro between their nests and the larvæ; and in no case did they take any notice of those sounds. But a condition which was possibly of an interfering nature was present, and it was necessary to eliminate it: "Thinking, "however, that they might perhaps be too much absorbed by "the idea of the larvæ to take any notice of my interruptions, "I took one or two ants at random and put them on a strip "of paper, the two ends of which were supported by pins "with their bases in water. The ants imprisoned under these "circumstances wandered slowly backwards and forwards "along the paper. As they did so I tested them in the same "manner as before, but was unable to perceive that they took "the slightest notice of any sound which I was able to pro-"duce" (*ibid.*, pp. 222-3).

(1) Experiments to test whether ants can hear sounds made by men.

It would thus seem to be demonstrated that ants cannot hear any sounds such as we hear. But there remains the possibility that they may hear others to which we are deaf. Can they, then, communicate by sound? To test this, Sir John tried several experiments:—"I put out "(Sept., 1874) on the board where one of my nests of "*Lasius flavus* was usually fed, six small pillars of wood "about an inch and a half high, and on one of them I put "some honey. A number of ants were wandering about on "the board itself in search of food, and the nest itself was "immediately above, and about 12 inches from, the board. "I then put three ants to the honey, and when each had "sufficiently fed I imprisoned her and put another; thus "always keeping three ants at the honey, but not allowing

(2) Experiments to test whether ants can communicate by sound.

"them to go home. If then they could summon their "friends by sound, there ought soon to be many ants at the "honey" (*ibid.*, p. 223). The results were then carefully watched and tabulated, and it was found that not more ants visited the honey "than would naturally result from the "numbers running about close by." But when the imprisoned ants were liberated and allowed to go home, there was a great change in this respect. In the four hours during which the ants who knew of the honey were imprisoned only seven others came, but in the three-quarters of an hour succeeding their liberation, when they had an opportunity to inform their friends of the store, fifty-four came. Similar results were obtained every time the experiment was worked. "It seems obvious, therefore," says Sir John, "that in these "cases no communication was transmitted by sound" (*ibid.*, pp. 224-5).

(3) Experiments to test whether ants produce sounds.

To test whether ants produce sounds "an extremely sen- "sitive microphone . . . was attached to the underside of "one of my nests; and though we could distinctly hear "the ants walking about, we could not distinguish any other "sound" (*ibid.*). There is, thus, much evidence tending to show that ants are deaf and dumb. But the caution of the true scientist is shown by Sir John Lubbock in not accepting all this negative evidence as conclusive. He, indeed, inclines to the opinion that "ants may produce sounds en- "tirely beyond our range of hearing" (*ibid.*). To this conclusion he is led by considerations of structure, especially of the antennæ and of the abdomen. In other words he will not allow that the negative evidence from experience overrides the presumption from the presence of positive conditions. The evidence is too technical to be here reproduced; but we may quote Sir John's summary of its general bearing: "It is indeed true that ants produce no sounds which "are audible by us; still, when we find that certain allied "insects do produce sounds appreciable to us by rubbing the "abdominal segments one over the other; and when we "find, in some ants, a nearly similar structure, it certainly "seems not unreasonable to conclude that these latter also

(4) Reasons for believing that ants can produce sounds.

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"do produce sounds even though we cannot hear them" (*ibid.*, p. 230). Such an example of investigation as this shows us again the difficulties which beset the investigator in attempting to establish even a comparatively simple fact, and it also makes abundantly plain the important part played by inference of a deductive character in such an investigation.

Experimental determination of the laws o the pendulum.

(*d*) *The Laws of the Pendulum.* We will now consider some typical investigations in which experiment could be appealed to throughout, and in which, consequently, the determination of the result could be much more exact. As a comparatively simple case we will take the establishment of the laws of the pendulum. Galileo, by experimenting on balls suspended by threads of different lengths, discovered that the time of oscillation depends upon the length, and is proportional to its square root. This was demonstrated more exactly by Newton. Reverting to the symbolism we used in the last section we may say that the R_1 of the problem might be represented by such a circumstance as the temperature, which being the same for each pendulum used throughout the experiment could not affect the result. The R_2 might embrace gravity, the resistance of the air, and the substance of which the pendulums were made. This R_2 must be analysed so as to eliminate what is inoperative and to leave the R_3 which is the sufficient ground (a) for variation in time (x). Newton made two pendulums of equal spherical wooden boxes suspended by strings of equal lengths. In the centre of these boxes were placed equal weights of various materials—first wood and gold, and then the experiment was repeated with silver, lead, glass, sand, common salt, water and wheat substituted for the gold. By securing similar shape, and equal size, and weight, the influence of the resistance of the air was eliminated, as all would suffer equal retardation. Newton found that the pendulums thus made oscillated for a length of time with equal oscillations, though he considered that a variation of a thousandth part would have been apparent. The material of which the pendulum was composed was thus shown to

(1) Establishment of the dependence of time of oscillation on length.

(2) Establishment of the reciprocal character of this dependence.

be an indifferent element, and it was established that all substances are equally accelerated by gravity. Length, which was already known to be operative in the time of oscillation was thus proved to be the *only* operative condition determining the time of oscillation at any one place, and the proposition connecting length with time was shown to be reciprocal. As the oscillation itself is due to gravity, its time would, of course vary with any variation in that force. Indeed, it is by means of this variation that differences in the force of gravity on the surface of the earth are quantitatively established, and that force is found to diminish as the equator is approached.

Investigations to decide between the hypotheses of (1) chemical action and (2) contact, as sources of power in a Voltaic Pile.

(*e*) *Source of Power in Voltaic Pile.* As another example, we will take Faraday's experimental proof of the theory that the source of power in a voltaic pile is due to chemical action. At the time he entered upon this investigation there were several hypotheses "but by far the most important are the two which respectively find the source of "power in contact, and in chemical force" (*Experimental Researches in Electricity*, vol. ii., § 1796). It is impossible to enumerate the enormous number of experiments Faraday made to settle this question. But the logical ground of them all was the same—to establish the hypothetical: 'If chemical action, then an electric current,' and its reciprocal 'If a current, then chemical action,' *i.e.*, 'If there is no chemical action, there is no current.' This he did by examining both positive and negative instances. By numerous experiments he established the positive connexion, and showed that chemical action is both efficacious in producing a current, and sufficient by itself, and without any contact, to do so. For example: "When tin was associated with "platinum, gold, or, I may say, any other metal which is "chemically inactive in the solution of the sulphuret [of "potassium], a strong electric current was produced," and as the chemical action decreased and finally ceased in consequence of the formation on the tin of an "insoluble, in-"vesting, non-conducting sulphuret of that metal," the electric current diminished and finally ceased also (*ibid.*,

(1) Experiments to prove positive connection between chemical action and current.

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§ 1882, *cf.* § 2031). Further, "when the chemical action "changes, the current changes also.—This is shown by the "cases of two pieces of the same active metal in the same "fluid. Thus if two pieces of silver be associated in strong "muriatic acid, first the one will be positive and then the "other; and the changes in the direction of the current will "not be slow as if by a gradual action, but exceedingly sharp "and sudden" (*ibid.*, § 2036).

(2) Negative experiments to establish the reciprocal proposition.

The negative experiments establishing the reciprocal proposition were also numerous—"Where no chemical action "occurs no current is produced.—This in regard to ordinary "solid conductors, is well known to be the case, as with "metals and other bodies. It has also been shown to be "true when fluid conductors (electrolytes) are used, in "every case where they exert no chemical action, though "such different substances as acid, alkalies and sulphurets "have been employed. . . . But a current will occur the "moment chemical action commences.—This proposition "may be well established by the following experiment. . . . "Two plates of iron and platinum are placed parallel, but "separated by a drop of strong nitric acid at each extremity. "Whilst in this state no current is produced . . .; but if "a drop of water be added . . . chemical action commences, "and a powerful current is produced, though without "metallic or other additional contact" (*ibid.*, §§ 2038-9).

Summary of results of experiments.

Faraday thus sums up: "With such a mass of evidence "for the efficacy and sufficiency of chemical action . . .; "with so many current circuits without metallic contact and "so many non-current circuits with; what reason can there "be for referring the effect in the joint cases where both "chemical action and contact occur, to contact, or to any-"thing but the chemical force alone? Such a reference "appears to me most unphilosophical: it is dismissing a "proved and active cause to receive in its place one which "is merely hypothetical" (*ibid.*, § 2053). This last sentence well illustrates the kind of hypothesis whose use was condemned by Newton [*see* § 145 (iv.)].

Such a brief sketch gives but a faint idea of the thorough-

ness with which Faraday tested the two rival hypotheses, or of the wealth of evidence in favour of the theory of chemical activity which he brought forward; but it is, perhaps, sufficient to illustrate the fundamental point that the logical character of the method is throughout an appeal to both positive and negative instances.

Establishment of the existence of Argon.

(*f*) *Argon.* As a final example, we will examine the researches of Lord Rayleigh and Professor Ramsay into argon, the newly discovered constituent of the atmosphere as recorded in the Abstract of their paper read before the Royal Society on January 31st, 1895, published in *Nature*, vol. li., pp. 347-356, from which our quotations are taken. This furnishes us with a very complete and beautiful illustration of the logical inductive method. The investigation started from the detection of an unexplained residual phenomenon. Careful determination of density had shown that nitrogen obtained from various chemical compounds is of a uniform density, but that 'atmospheric' nitrogen is about $\frac{1}{2}$ per cent. heavier. Two hypotheses—each of which placed the explanation in the character of the lighter 'chemical' nitrogen, and were suggested by analogy drawn from experience in the chemical laboratory—were successively conceived to account for this phenomenon, and were rejected after being tested. "When the discrepancy of "weights was first encountered, attempts were naturally "made to explain it by contamination with known im- "purities. Of these the most likely appeared to be hydrogen, "present in the lighter gas in spite of the passage over red- "hot cupric oxide. But inasmuch as the intentional intro- "duction of hydrogen into the heavier gas, afterwards treated "in the same way with cupric oxide, had no effect upon its "weight, this explanation had to be abandoned, and finally "it became clear that the difference could not be accounted "for by the presence of any known impurity. At this stage "it seemed not improbable that the lightness of the gas "extracted from chemical compounds was to be explained "by partial dissociation of nitrogen molecules N_2 into "detached atoms. In order to test this suggestion both

(1) The investigation was suggested by a residual phenomenon.

(2) Rejection of first hypothesis—contamination.

(3) Rejection of second hypothesis—dissociation.

"kinds of gas were submitted to the action of the silent "electric discharge, with the result that both retained their "weights unaltered. This was discouraging, and a further "experiment pointed still more markedly in the negative "direction. The chemical behaviour of nitrogen is such "as to suggest that dissociated atoms would possess a high "degree of activity, and that even though they might be "formed in the first instance their life would probably be "short. On standing they might be expected to disappear, "in partial analogy with the known behaviour of ozone. "With this idea in view, a sample of chemically prepared "nitrogen was stored for eight months. But at the end of "this time the density showed no sign of increase, remaining "exactly as at first" (p. 348 *b*).

(4) Adoption of third hypothesis —existence of new constituent of atmosphere.

Another hypothesis had, therefore, to be sought on the established ground that "one or other of the gases must be a "mixture." But as, "except upon the already discredited "hypothesis of dissociation, it was difficult to see how the gas "of chemical origin could be a mixture . . . the simplest ex-"planation in many respects was to admit the existence of a "second ingredient in air from which oxygen, moisture, and "carbonic anhydride had already been removed" (p. 349 *a*).

(5) Exception shown to be only apparent, and really to support the hypothesis.

This hypothesis, however, is immediately met by an objection which partially takes the form of a logical exception. "In "accepting this explanation, even provisionally, we had to "face the improbability that a gas surrounding us on all "sides, and present in enormous quantities, could have "remained so long unsuspected" (*ibid.*). The answer to this objection was only fully found at a much later period of the investigation, when it was shown to be only an apparent objection, as the inert character of the newly discovered gas sufficiently accounts for its having so long escaped observation. It was necessary, however, to examine at once "the "evidence in favour of the prevalent doctrine that the inert "residue from air after withdrawal of oxygen, water, and "carbonic anhydride, is all of one kind," as such evidence was logically an exception to the new hypothesis, and, therefore, if sustained, must prove fatal to it. This evidence

rested upon the experiments of Cavendish, "whose method "consisted in operating with electric sparks upon a short "column of gas confined with potash over mercury at the "upper end of an inverted U tube" (*ibid.*). But it appears that these very experiments are evidence in favour of, instead of in opposition to, the hypothesis of an additional constituent in the atmosphere, for Cavendish records that "a small "bubble of air remained unabsorbed" at the end of his experiments. The exception is, therefore, only apparent, and when exactly stated, helps 'to prove the rule.'

(6) Positive experiments to prove existence of Argon in 'atmospheric' nitrogen.

The ground being thus cleared, experiments were instituted to establish the new hypothesis. These were, of course, both positive and negative, the former being directed to establish the existence of a new constituent in 'atmospheric' nitrogen, and the latter to prove that this constituent is absent from 'chemical' nitrogen. The positive experiments, in other words, were directed to establishing the proposition 'If the excess of density of 'atmospheric' over 'chemical' nitrogen is due to the presence in the former of another gas besides nitrogen, then this other gas will remain after the nitrogen is withdrawn,' and the negative experiments to showing that this residue cannot be nitrogen, as it is not found when 'chemical' nitrogen is the gas operated upon.

The first mode of positive experiment was sparking air to which oxygen was gradually added in the presence of an alkali—*i.e.* the method of Cavendish mentioned above. In every case there was a small residue, and "that this small "residue could not be nitrogen was argued from the fact "that it had withstood the prolonged action of the spark, "although mixed with oxygen in nearly the most favourable "proportion" (p. 349 *b*).

Function of number of instances.

The next step illustrates the function of number of instances in experiment, viz. to ensure that extraneous elements have not crept in, *i.e.* that only known conditions are present. It is thus described: "Although it seemed "almost impossible that these residues could be either "nitrogen or hydrogen, some anxiety was not unnatural, "seeing that the final sparking took place under somewhat

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"abnormal conditions. The space was very restricted, and "the temperature (and with it the proportion of aqueous "vapour) was unduly high. But any doubts that were felt "upon this score were removed by comparison experiments "in which the whole quantity of gas operated on was very "small. . . . Several repetitions having given similar results, "it became clear that the final residue did not depend upon "anything that might happen when sparks passed through a "greatly reduced volume, *but was in proportion to the amount* "*of air operated upon*" (pp. 349 *b*—350 *a*).

The second mode of positive experiment was to pass 'atmospheric' nitrogen over red-hot magnesium. A residue was left, and "on passing sparks for several hours through "a mixture of a small quantity of this gas with oxygen, its "volume was still further reduced. Assuming that this "reduction was due to the further elimination of nitrogen, "the density of the remaining gas was calculated to be 20·0" (p. 350 *a*).

The third mode of positive experiment was by atmolysis, or the transmission of 'atmospheric' nitrogen through a porous substance in order to determine whether it is a mixture of gases of different densities. When such a mixture is thus treated the lighter gas passes through the porous substance first. When air had, therefore, undergone atmolysis, and the oxygen, ammonia, moisture and carbonic anhydride been removed from that portion which had not passed through the porous substance, the remaining 'atmospheric' nitrogen should—on the hypothesis adopted for investigation —be of a greater density than ordinary 'atmospheric' nitrogen, as it should contain a larger proportion of the denser gas, and this was found to be the case in each of three such experiments. The positive experiments, therefore, appear to yield overwhelming evidence that 'atmospheric' nitrogen is a mixture of 'chemical' nitrogen and a gas of greater density, hitherto unknown, and to which the discoverers give the name *Argon*.

(7) Negative experiments to prove absence of Argon from 'chemical' nitrogen.

A series of negative experiments was then entered upon, in which each of the first two series of positive experiments

was performed upon 'chemical' instead of 'atmospheric' nitrogen. In each case a very small residue of argon was found, from one-ninth to one-fortieth of the amount that would have remained had 'atmospheric' nitrogen been used. Even this small residue could be accounted for. In the cases in which it was largest there had been leakage and air had entered, and in the others "the source of the residual "argon is to be sought in the water used for the manipula-"tion of the large quantities of gas . . . employed." This is supported by analogy: "When carbonic acid was collected "in a similar manner, and subsequently absorbed by potash, "it was found to have acquired a contamination consistent "with this explanation" (p. 350 *b*).

Apparent exception accounted for.

The negative experiments thus corroborate the positive ones, and the existence of argon is proved; thus the hypothesis is established and the first induction is completed.

Investigations into the nature of Argon.

The next object was to examine the nature of argon, and such an examination yielded many confirmations of the results already obtained, by showing that argon differs radically from nitrogen.

Determination of Density.

The first problem was to determine its density. This was first reached by deductive reasoning "on the assumption that "the accurately known densities of atmospheric and of "chemical nitrogen differ on account of the presence of "argon in the former, and that during the treatment with "oxygen nothing is oxidized except nitrogen" (p. 351 *b*). This assumption was known to be justified because experiment had proved that the density of 'chemical' nitrogen obtained from magnesium nitride, which had been prepared by passing 'atmospheric' nitrogen over ignited magnesium was of the usual density of chemical nitrogen. "It is there-"fore seen that 'chemical' nitrogen, derived from 'atmo-"spheric' nitrogen, without any exposure to red-hot copper, "possesses the usual density" (p. 348). The density thus obtained was 20·6. Here again we have the suggestion of a hypothesis by the quantitative computation of a residual phenomenon. This hypothesis was then to be tested experimentally. This could not be done by direct weighing, as it

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was not found possible to collect by the oxygen method enough of the gas to fill the large globe employed for other gases. So a mixture of about 400 c.c. of argon with pure oxygen was weighed, and the density of argon calculated from the result, and found to be 19·7. The argon derived from the second series of positive experiments was found to have a density of 19·9. As the density of argon is thus proved to be different from that of nitrogen, this determination supports the conclusion already reached that argon is a distinct constituent of atmospheric nitrogen.

Examination of spectra,

A careful examination of the spectrum of argon by Mr. W. Crookes disclosed the fact that argon "gives two "distinct spectra according to the strength of the induction "current employed." Nitrogen does the same, "but while "the two spectra of nitrogen are different in character, one "showed fluted bands and the other sharp lines, the argon "spectra both consist of sharp lines" (p. 354 *a*). Additional confirmation of the existence of argon was thus furnished. But further, Mr. Crookes says: "I have found no other "spectrum-giving gas or vapour yield spectra at all like those "of argon" (p. 354 *a*), and this is a proof that argon is a gas previously unknown. But the fact that it yields two spectra suggested the hypothesis that argon is really a mixture of two gases. This is an instance of how the solution of one problem opens up others by bringing to light new phenomena requiring explanation. Other considerations bearing on this point will be noticed later on.

and suggestion of doubt as to whether Argon is a simple gas or a mixture.

Determination of solubility in water.

The solubility of argon in water was next determined, and was found to be approximately that of oxygen and about two and a half times that of nitrogen. This conclusion was proved by testing the deductive inference that, if so, argon would be found in increased proportion in the dissolved gases of rain water; the test experiment showed that the argon in 'water' nitrogen is in the ratio of 24 : 11 to that in 'atmospheric' nitrogen.

Experiments on liquefaction and solidification.

Experiments were conducted by Professor Olszewski on the liquefaction and solidification of argon. "Four series "of experiments in all were carried out, two with the object

"of determining the critical temperature and pressure of "argon, as well as measuring its vapour pressure at several "other low temperatures, while two other series served to "determine its boiling and freezing points under atmospheric "pressure, as well as its density at its boiling point" (p. 355 *a*). It was found that the critical temperature is $-121°{\cdot}0$ (that of nitrogen being $-146°{\cdot}0$), its critical pressure 50·6 atmospheres (that of nitrogen being 35·0), its boiling point $-187°{\cdot}0$ (that of nitrogen being $-194°{\cdot}4$), and its freezing point $-189°{\cdot}6$, at which temperature it solidifies into white crystals (that of nitrogen being $-214°{\cdot}0$).

Argon proved to be monatomic and therefore not a compound.

It further appears from experiments made on the velocity of sound in argon "that argon gives practically the ratio of "specific heats, viz., 1·66, proper to a gas in which all the "energy is translational. The only other gas which has been "found to behave similarly is mercury gas, at a high tem-"perature" (p. 352 *b*). "In the case of mercury the absence "of interatomic energy is regarded as a proof of the mon-"atomic character of the vapour, and the conclusion holds "equally good for argon. . . . Now a monatomic gas can "only be an element, or a mixture of elements; and hence "it follows that argon is not of a compound nature" (p. 353).

It remains doubtful whether argon is a simple gas or a mixture.

But whether it is an element or a mixture of elements is still doubtful. "There is evidence both for and against the "hypothesis that argon is a mixture: for, owing to Mr. "Crookes' observations of the dual character of its spectrum; "against, because of Prof. Olszewski's statement that it has "a definite melting point, a definite boiling point, and a "definite critical temperature and pressure; and because on "compressing the gas in presence of its liquid, pressure "remains sensibly constant until all gas has condensed to "liquid. The latter experiments are the well-known criteria "of a pure substance; the former is not known with cer-"tainty to be characteristic of a mixture. . . . For the "present, however, the balance of evidence seems to point "to simplicity" (p. 353 *b*).

The atomic weight of argon.

If argon is an element its atomic weight is 40, for its molecule is identical with its atom, and the molecular weight

of a gas is double its density, which in the case of argon is approximately 20.

Argon is extremely inert.

Many attempts were made to induce argon to combine with other substances, but they all proved abortive. Since the publication of the paper we are quoting from, however, M. Berthelot claims to have established that under the influence of electricity argon enters into chemical action with benzene vapour. If this is the case, it is the first chemical action in which argon has been observed to take part, and the gas is, therefore, of an extremely inert character.

Its inertness accounts for its late discovery.

Finally, the determination of the nature of argon as far as it has gone answers the objection to the hypothesis of the existence of argon drawn from previous non-observation. "It need excite no astonishment that "argon is so indifferent to reagents. For mercury, although "a monatomic element, forms compounds which are by no "means stable at a high temperature in the gaseous state; "and attempts to produce compounds of argon may be "likened to attempts to cause combination between mercury "gas at 800° and other elements" (p. 353 *b*). "Its inertness, "which has suggested its name, sufficiently explains why it "has not previously been discovered as a constituent of com-"pound bodies" (p. 354 *a*). Since the publication of the paper, however, Professor Ramsay and Mr. Crookes have found that a gas obtained from a mineral called cleveite is a mixture of argon and helium, a most interesting discovery, as helium was hitherto only known as abundant in the surroundings of the sun. On the other hand, it has been ascertained that there is no argon in mice or peas—these being chosen as representatives of the animal and vegetable kingdoms respectively.

The above investigations illustrate fully the doctrine of induction.

Even such an imperfect outline as the above makes abundantly manifest that induction is by no means an easy process, or one that can be reduced to mechanical rules; that the procedure starts from, and is guided throughout by hypotheses; that number of experiments is appealed to only as a guarantee that only known conditions are operative; that the procedure of perceptual analysis is to establish a

positive connexion, to purge this of exceptions and to limit and corroborate it by negative instances; and that one inductive enquiry gives rise to others. In brief, this discovery of first-rate scientific importance illustrates in every point the doctrine of induction set forth in this and the preceding chapters.

*155. Mill's 'Experimental' or 'Inductive' Methods.

As Mill's Methods have obtained general currency they demand some examination.

Although we regard the preceding exposition as a complete statement of the logical principles which guide all qualitative analysis of the given, and of the general character of the method adopted, yet a discussion of this subject in an English treatise on Logic would probably be considered incomplete if it did not embrace a consideration of the "Four "Methods of Experimental Enquiry" or "Methods of "Direct Induction," as they are indifferently called, to which Mill's *Logic* has given currency and authority. To this, then, we will now briefly address ourselves.

(i.) Statement of the Methods.

Mill regards these methods as modes of reaching general laws by seeking resemblances between phenomena.

The exposition of these "methods" occupies a foremost place in Mill's system, and follows his discussion of the preliminary doctrines of the uniformity of nature and causation. The methods themselves are based upon the popular idea of causation, as an invariable sequence between phenomena, with which Mill sets out. "Cause" is at first regarded as a physical thing, though it is afterwards confined to some particular property of a thing. The inductive problem takes the form of so analysing the course of observed events that it becomes possible to gather from an examination of them universal propositions about their succession. Throughout runs the fundamental assumption of empiricism, that all inference is based upon perceptible resemblance [*cf.* §§ 138 (i.)[1]; 145 (v.)], and that, consequently, all instances of the same causal sequence will have points of resemblance both in the antecedent and in the consequent phenomena. Hence, there are two "obvious "modes of singling out from among the circumstances which "precede or follow a phenomenon, those with which it is

Hence, the two main Methods are those of *Agreement* and *Difference*,

[1] First Edition, § 155 (i.).

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"really connected by an invariable law. . . . One is, by "comparing together different instances in which the "phenomenon occurs. The other is, by comparing instances "in which the phenomenon does occur, with instances in "other respects similar in which it does not. These two "methods may be respectively denominated, the Method of "Agreement, and the Method of Difference" (Mill, *Logic*, III., viii., § 1). The other two methods given by Mill as independent—those of "Residues" and of "Concomitant "Variations"—are really but particular cases of these. Mill gives, moreover, a fifth method, though he does not recognize it as independent, but as merely a double application of the method of agreement, which he styles the "Joint "Method of Agreement and Difference" or the "Indirect "Method of Difference."

of which all others are modifications.

All these methods are gathered from Herschel's *Preliminary Discourse on the Study of Natural Philosophy* "in "which alone," says Mill, "of all books which I have met "with, the four methods of induction are distinctly recog-"nized, though not so clearly characterized and defined, nor "their correlation so fully shown, as has appeared to me desir-"able" (*Logic*, III., ix., § 3). Mill, therefore, to repair this imperfection, formulated the four [or five] methods with considerable show of precision in the following five "Canons of "Induction" (*ibid.*, viii.), and, denoting "antecedents by the "large letters of the alphabet, and the consequents corre-"sponding to them by the small" (*ibid.*, § 1), also gave symbolic formulæ to express the chief methods:—

Mill gathered the Methods from Herschel, but formulated them more precisely.

Statement of Mill's Canons:—

First Canon (**Method of Agreement**).

(1) Agreement;

"If two or more instances of the phenomenon under "investigation have only one circumstance in common, "the circumstance in which alone all the instances "agree, is the cause (or effect) of the given pheno-"menon."

Symbolic formula: ABC—abc; ADE—ade ∴ A—a is causal sequence.

Second Canon (**Method of Difference**).

(2) Difference;

"If an instance in which the phenomenon under investi-"gation occurs, and an instance in which it does not "occur, have every circumstance in common save one, "that one occurring only in the former; the circum-"stance in which alone the two instances differ, is the "effect, or the cause, or an indispensable part of the "cause, of the phenomenon."

Symbolic formula: *ABC—abc; BC—bc* ∴ *A—a* is causal sequence.

Third Canon (**Joint Method**).

(3) Joint Agreement and Difference;

"If two or more instances in which the phenomenon "occurs have only one circumstance in common, while "two or more instances in which it does not occur "have nothing in common save the absence of that "circumstance; the circumstance in which alone the "two sets of instances differ, is the effect, or the "cause, or an indispensable part of the cause, of the "phenomenon."

Symbolic formula. Mill gives no formula for this method, and several have been suggested which more or less fail to fulfil the conditions of the canon. Bearing in mind that, as Dr. Fowler puts it (*Ind. Log.*, p. 160), "the positive and negative instances must be *in pari* "*materiâ*," the formula might, on the lines of those already given, be thus expressed:

ABC—abc, ADE—ade; BDM—bdm, CEO—ceo ∴ *A—a* is a causal sequence.

Fourth Canon (**Method of Residues**).

(4) Residues;

"Subduct from any phenomenon such part as is known "by previous inductions to be the effect of certain "antecedents, and the residue of the phenomenon is "the effect of the remaining antecedents."

Symbolic formula: If *A—a*, *B—b* are known to be causal sequences, then, given *ABC—abc*, it can be inferred that *C—c* is also a causal sequence.

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(5) Concomitant Variations.

Fifth Canon (**Method of Concomitant Variations**).

"Whatever phenomenon varies in any manner whenever "another phenomenon varies in some particular "manner, is either a cause or an effect of that pheno- "menon, or is connected with it through some fact of "causation."

The symbolic formula would appear to be $A_1BC—a_1bc$; $A_2BC—a_2bc$; $A_3BC—a_3bc$; ∴ $A—a$ is a causal sequence.

Mill's Examples:— (1) of Agreement;

As examples of the *Method of Agreement*, Mill gives: "Let the antecedent A be the contact of an alkaline substance "and an oil. This combination being tried under several "varieties of circumstances, resembling each other in nothing "else, the results agree in the production of a greasy and "detersive or saponaceous substance: it is therefore con- "cluded that the combination of an oil and an alkali causes "the production of soap. It is thus we enquire, by the "Method of Agreement, into the effect of a given cause. In "a similar manner we may enquire into the cause of a given "effect. . . . For example, let the effect a be crystallization. "We compare instances in which bodies are known to assume "crystalline structure, but which have no other point of agree- "ment; and we find them to have one, and as far as we can "observe, only one, antecedent in common: the deposition "of a solid matter from a liquid state, either a state of "fusion or of solution. We conclude, therefore, that the "solidification of a substance from a liquid state is an "invariable antecedent of its crystallization" (*ibid.*, § 1).

(2) of Difference;

Of the *Method of Difference* Mill says: "It is scarcely "necessary to give examples of a logical process to which we "owe almost all the inductive conclusions we draw in early "life. When a man is shot through the heart, it is by this "method that we know that it was the gun-shot which "killed him: for he was in the fulness of life immediately "before, all circumstances being the same, except the wound" (*ibid.*, § 2).

(3) of Joint Method;

As an example of the *Joint Method of Agreement and Difference*, Mill takes one of the steps in Dr. Wells' investi-

gation into the formation of dew: "It appears that the "instances in which much dew is deposited, which are very "various, agree in this, and, so far as we are able to observe, "in this only, that they either radiate heat rapidly or conduct "it slowly: qualities between which there is no other cir-"cumstance of agreement, than that by virtue of either, the "body tends to lose heat from the surface more rapidly than "it can be restored from within. The instances, on the con-"trary, in which no dew, or but a small quantity of it, is "formed, and which are also extremely various, agree (so far "as we can observe) in nothing except in *not* having this "same property. We seem, therefore, to have detected the "characteristic difference between the substances on which "dew is produced, and those on which it is not produced. "And thus have been realized the requisitions of what we "have termed the Indirect Method of Difference, or the "Joint Method of Agreement and Difference" (III., ix., § 3). Book V. Ch. V.

(4) of Residues;

In illustration of the *Method of Residues* Mill quotes from Herschel several instances from various sciences. For example: "Many of the new elements of chemistry have "been detected in the investigation of residual phenomena. "Thus Arfwedson discovered lithia by perceiving an excess "of weight in the sulphate produced from a small portion of "what he considered as magnesia present in a mineral he "had analysed" (quoted from *Nat. Phil.*, § 161). "'Almost "all the greatest discoveries in Astronomy,' says the same "author, 'have resulted from the consideration of residual "phenomena of a quantitative or numerical kind. . . . It "was thus that the grand discovery of the precession of the "equinoxes resulted as a residual phenomenon, from the "imperfect explanation of the return of the seasons by the "return of the sun to the same apparent place among the "fixed stars'" (*Outlines of Astronomy*, § 856, quoted by Mill, III., ix., § 5).

(5) of Concomitant Variations.

Amongst examples of the *Method of Concomitant Variations* Mill takes the doctrine of inertia: "The simple oscillation of "a weight suspended from a fixed point, and moved a little "out of the perpendicular, which in ordinary circumstances

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"lasts but a few minutes, was prolonged in Borda's experi-"ments to more than thirty hours, by diminishing as much "as possible the friction at the point of suspension, and by "making the body oscillate in a space exhausted as nearly as "possible of its air. There could therefore be no hesitation "in assigning the whole of the retardation of motion to the "influence of the obstacles; and since, after subducting this "retardation from the total phenomenon, the remainder was "an uniform velocity, the result was the proposition known "as the first law of motion" (III., viii., § 7).

Mill discusses other and more complex examples, in which he finds several of the methods exemplified, but the above are sufficient to show the kind of investigation of nature he contemplated.

(ii.) Examination of the Methods.

Mill claims that the methods are (1) The only means of experimental inquiry;

For these methods Mill makes very high claims. They are "the only possible modes of experimental enquiry—of "direct induction *à posteriori*, as distinguished from deduc-"tion" (III., viii., § 7). "In saying that no discoveries were "ever made by the Four Methods, he [Whewell] affirms "that none were ever made by observation and experiment; "for assuredly if any were, it was by processes reducible "to one or other of those methods" (III., ix., § 6). But this is not all. "Induction is proof; it is inferring some-"thing unobserved from something observed: it requires, "therefore, an appropriate test of proof; and to provide "that test, is the special purpose of inductive logic" (III., ii., § 5). "The business of Inductive Logic is to pro-"vide rules and models (such as the Syllogism and its rules "are for ratiocination), to which if inductive arguments "conform, those arguments are conclusive, and not other-"wise. This is what the Four Methods profess to be" (III., ix., § 6).

(2) Methods of Proof.

These claims are by no means universally granted by logicians. As Mr. Bradley says: "The balance of authority "among modern logicians is, I think, against the claims of "the inductive proofs, and is not on their side" (*Prin. of*

Log., p. 331). Indeed, as will appear in the sequel, Mill himself does not consistently maintain these high claims, and abundantly shows that the canons both demand the unattainable, and fail to give conclusive proofs of those general propositions which he held it to be the business of induction to discover and prove (*cf.* III., i., § 2).

Mill's symbolism is misleading.

In considering the methods, and their claims to be inductive proofs, it is well at the outset to draw attention to the unfortunate character of Mill's symbolism. The use of *A, a; B, b; C, c;* &c., suggests that the causal connexions sought are already obtained; and this suggestion is strengthened by Mill's speaking of *a, b, c* as "the consequents *corresponding* "to" the antecedents *A, B, C*. This would be an open begging of the question. Mill probably does not mean this, but intends his symbols as mere empty forms. They are, however, undoubtedly confusing; if the correspondence between large and small letters means nothing, it should not be employed; whilst if it has meaning, it begs the whole question.

All the methods are based on the principle of elimination.

It must next be noticed that the methods bear a strong family likeness to each other. They are all based on the same principle. Both Agreement and Difference, Mill tells us, "are methods of *elimination*. . . . The Method of Agree-"ment stands on the ground that whatever can be eliminated, "is not connected with the phenomenon by any law. The "Method of Difference has for its foundation, that what-"ever cannot be eliminated, is connected with the pheno-"menon by a law" (III., viii., § 3). The Joint Method is "a double employment of the Method of Agreement" (*ibid.*, § 4). "The Method of Residues is . . . a peculiar "modification of the Method of Difference" (*ibid.*, § 5). The Method of Concomitant Variations is "but a modifica-"tion either of the Method of Agreement or of the Method "of Difference" (III., xxii., § 4). The methods might, indeed, all be expressed by the formula *AB—xy, AC—xz*, which avoids the objection just urged to Mill's symbolic statements. Here, as *B* can be removed without affecting *x*, it is assumed that *B* and *x* are not causally connected; whilst

as the removal of *B* is attended by the removal of *y*, it is assumed that *B*—*y* is a causal sequence. This represents the mode of proceeding by each method. Each, by removing parts of a complex whole, seeks to establish a relation between the remaining parts. Thus the methods are at bottom, as Mr. Bradley points out, "all of them Methods of Residues "or Methods of Difference" (*op. cit.*, p. 337).

The methods are not 'inductive' in the sense of inference from particulars,

We must now examine the claims of the methods to be called "inductive." By induction Mill continually tells us he means inference from particulars. It is needless to multiply quotations: two or three will suffice as examples. "All processes of thought in which the ultimate premises "are particulars . . . are . . . Induction" (II., iii., § 7). "Induction . . . is that operation of the mind, by which "we infer that what we know to be true in a particular case "or cases, will be true in all cases which resemble the former "in certain assignable respects" (III., ii., § 1). "The experi-"mental method . . . makes its experiments directly upon the "complex case" (III., x., § 8). But, if we examine the canons of the methods we find they do not even profess to start from particular complex facts. They demand that the "instances" "have only one circumstance in common," "have every circumstance in common save one," "have "nothing in common save the absence of that circumstance." Very little consideration is needed to show that the complex particular facts of experience can never fulfil such conditions as these. Such facts never agree, still less do they differ, in one point only. The canons assume that many circumstances are first dismissed from consideration as "already known to "be immaterial to the result" (III., viii., § 3). That is, they assume that the complex particulars of experience have already been analysed, and that limited groups of antecedents and consequents, known to be causally connected, have been separated out for the purpose of the "inductive" enquiry, whose task is only to obtain simpler causal connexions by eliminating some of the elements still left. Thus, as Mr. Laurie says in an able critical article on the methods in *Mind* (New Series, vol. ii.): "The position from which we are thus

for they all start with universals.

"invited to set out is very far from the beginning of experi-"mental enquiry; the clear-cut instances supposed are possible "only in an advanced stage of scientific research" (p. 324). Whewell is, indeed, quite justified when he says: "Upon "these methods, the obvious thing to remark is, that they "take for granted the very thing which is most difficult to "discover, the reduction of the phenomena to formulæ such "as are here presented to us" (*Phil. of Discovery*, p. 263). Put in other words, the methods can only start with universal judgments; for it is just that analysis and elimination which the methods presuppose which constitute the universal. As Mr. Bradley says in his searching criticism of the methods: "The moment you have reduced your particular fact to a "perfectly definite set of elements, existing in relations "which are accurately known, there you have left the fact "behind you. You have already a judgment universal in "the same sense in which the result of your 'induction' is "universal" (*op. cit.*, p. 335). The demand for a universal to start from is, indeed, made explicitly by the Fifth Canon which speaks of one phenomenon varying *whenever* another varies, thus postulating that a universal connexion between the two should be established before the method can be brought to bear upon it. Hence, the methods which are held out to us "as the only possible modes of experimental en-"quiry—of direct induction *à posteriori*" (Mill, III., viii., § 7) —are seen to presuppose the very work which they themselves are set forth as alone capable of accomplishing.

Mill's examples of starting from real particulars do not fulfil the requirements of his canons.

Mill does, indeed, give two instances in which he applies the methods to the actual raw material of experience. "If it "had been my object," he says, "to justify the processes "themselves as means of investigation, there would have "been no need to look far off, or make use of recondite or "complicated instances. As a specimen of a truth ascertained "by the Method of Agreement, I might have chosen the "proposition 'Dogs bark.' This dog, and that dog, and the "other dog, answer to *ABC*, *ADE*, *AFG*. The circumstance "of being a dog answers to *A*. Barking answers to *a*. As a "truth made known by the Method of Difference, 'Fire

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"burns' might have sufficed. Before I touch the fire I am "not burned; this is BC; I touch it and am burnt; this is "ABC, aBC" (III., ix., § 6). On this we cannot do better than quote Mr. Bradley's brief but sufficient criticism: "The Canons we think are not hard to content if this will "satisfy them. But surely their author had forgotten them "for the moment. By seeing three barking dogs I perceive "that they '*have only one circumstance in common.*' By "standing in front of a burning fireplace, and then touching "the fire and being burnt, I am to know that the two facts "'*have every circumstance in common but one.*' Is not this "preposterous? Surely it is clear in the first case that Mr. "Mill's way of arguing might prove just as well that all dogs "have the mange, and in the second that every fireplace "blisters. And these conclusions hardly seem to be sound." And in a note he points out: "As a test of the writer's "accuracy in small points, we may notice that in the second "example there is a mistake in the working of the Method. "The right conclusion is 'Touching burns'; for the fire is "not the differential condition. It was there before I "touched it, and if it was not there, then we have *two* differ- "ences and another kind of mistake" (*op. cit.*, p. 336). It is seen, then, that the methods are not 'inductive,' in Mill's sense of the term, for they do not start from particular facts, but from propositions as universal as those they profess to prove. Moreover, as Mr. Bradley says: "The Methods of induction "are placed in this dilemma. Because they *presuppose* uni- "versal truths, therefore they are not the only way of "proving them. But if they are the only way of proving "them, then every universal truth is unproved" (*op. cit.*, pp. 336-7).

The reasoning involved by the methods is deductive,

In the next place, if we examine the kind of reasoning which the methods involve, we find that it is deductive throughout. This is apparent at the first glance in the Method of Residues, and is acknowledged by Mill: "Of "the two instances . . . which the Method of Difference "requires,—the one positive, the other negative,—the nega- "tive one, or that in which the given phenomenon is absent,

"is not the direct result of observation and experiment, but "has been arrived at by deduction" (III., viii., § 5). But, in so far as inference is involved in the use of any one of the methods, it is deductive in its essence. As was said above all the methods can be fairly represented by the formula $AB—xy$; $AC—xz$. The argument then runs—

Any antecedent is invariably followed by the same consequent,
A is followed in this case by x and y (1st example),
∴ A is invariably followed by x or y.

Taking this as the next major premise we have—

A is invariably followed by x or y,
But A is not invariably followed by y (2nd example),
∴ A is invariably followed by x;
i.e., x is the effect of A.

By similar arguments it can be established from the premises that A is the cause of x, and that $B—y$, $C—z$ are also reciprocal causal connexions. The type of argument is seen to be deductive throughout, and to be valid if the truth and sufficiency of the premises are granted, and if A, B, C, and x, y, z, symbolize elements which are independent of each other. But in no case is the inference "inductive" in the sense of drawing a conclusion more general than the premises.

and the conclusion is not more general than the premises.

If the methods do not agree with Mill's conception of induction, it remains to enquire into their validity as proofs. "To ascertain . . . what are the laws of causation which "exist in nature; to determine the effect of every cause, "and the causes of all effects,—is the main business of "Induction; and to point out how this is done is the chief "object of Inductive Logic" says Mill (III., vi., § 3). The object of the methods, therefore, is to establish laws of causation. But "The Law of Causation . . . is but the "familiar truth, that invariability of succession is found by "observation to obtain between every fact in nature and "some other fact which has preceded it" (III., v., § 2).

The methods aim at establishing laws of causation,

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yet they assume causation.

Yet, when we look at the canons, we find that they can only work if the causal connexion of the antecedents as a whole and the consequents as a whole is presupposed; for it is obvious that "invariability of sequence" cannot be established by an examination of "*two* or more instances." Indeed, Mill himself supplies us with abundant evidence that the methods can never prove any general law.

Mill himself says the methods are based on false assumptions,

After fully explaining and illustrating the methods (III., viii., ix.), he calmly tells us that the whole discussion has proceeded upon false suppositions. "We have regarded "*a b c d e*, the aggregate of the phenomena existing at any "moment, as consisting of dissimilar facts *a, b, c, d,* and *e,* "for each of which one, and only one, cause needs be sought. ". . . But the supposition does not hold, in either of its "parts. In the first place, it is not true that the same "phenomenon is always produced by the same cause. . . . "And, secondly, the effects of different causes are often "not dissimilar, but homogeneous, and marked out by no "assignable boundaries from one another" (III., x., § 1). Grave doubt is thus thrown on the methods as a whole, yet the high claims previously advanced for them are not withdrawn. But Mill does not simply undermine their authority generally; he practically destroys that of each one separately.

and grants that Agreement can never yield proof.

The Method of Agreement stands as one of these methods of "proof"—and sets out with claiming to establish both the effect of a given cause, and the cause of a given effect. But immediately after, we are told that the conclusion by this method "remains subject to very considerable doubt. . . . "This uncertainty arises from the impossibility of assuring "ourselves that *A* is the *only* immediate antecedent common "to both the instances" (III., viii., § 1). In other words it is impossible to fulfil the requirements of the canon. But further, plurality of causes is "a characteristic imperfection "of the Method of Agreement" (III., x., § 2), which is essentially a method of observation as distinguished from experiment. Now, plurality of causes is, as has been shown [*see* § 143 (ii.) (*c*)] a false doctrine, if 'cause' and 'effect' are

interpreted strictly, and the phenomena fully analysed. But in the rough sense in which Mill is using the word 'cause,' the possibility of such plurality must be granted; as Mill says, "many causes may produce death." But once grant the plurality of causes and the method ceases to furnish any valid proof. This Mill insists on over and over again. For example, he says: "If there are but two instances, *ABC* and *ADE*, though "these instances have no antecedent in common except *A*, yet, "as the effect may possibly have been produced in the two "cases by different causes, the result is at most only a slight "probability in favour of *A*" (III., x., § 2). Hence, the First Canon is shown by its author to be false as the statement of a method of proof. But Mill goes even further in the work of destruction. He tells us to multiply and vary our instances "all agreeing in no other antecedent except *A*"—a requirement which he has already told us it is impossible to fulfil—as the only means of increasing the probability of our conclusion. But even then the method fails us; "by that method alone we never can arrive at causes" (III., xvi., § 5). Thus, Agreement is shown to be a "method of proof" which proves nothing. Indeed, to look for perceptible resemblance amongst the various antecedents of such events as Mill designates as causes is futile. As Sigwart says: "Mill's own "example of crystallization shows the inadequacy of the "methods, for if we ask what is common to the states from "which crystalline bodies issue, we find that there is ulti- "mately nothing but corporeality in general, since crystalline "structure is formed from gaseous, fluid and amorphous "solid states of the most various substances; the method "of agreement would show as the invariable antecedent of "this phenomenon only some material substance, and for "this there was no need of a complicated method. The "result would be similar if we conducted our enquiry as to "the cause of death of a living being; the instances in which "a living being dies have no antecedent in common—at any "rate for the one way of investigating antecedents—except "life, and life would appear at the end of the elimination as "the cause of death" (*Logic*, Eng. Tr., vol. ii., p. 341).

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Mill regards Difference as the most important method,

The Method of Difference is, however, regarded by Mill as the most valuable of all the inductive methods; though, were they really methods of *proof*, there would, of course, be no superiority in one of them over the others, and though, as we have seen, they are all based on the same principle. It is essentially the method of experiment, and it is claimed that by its agency both cause and effect can be established.

and as alone capable of proving causation.

Indeed, it is "by the Method of Difference alone that we "can ever, in the way of direct experience, arrive with cer- "tainty at causes" (III., viii., § 3). Yet, in the very example which Mill selects of a man shot through the heart, it is evident that there is no inference to cause. There is nothing to justify the assertion that if a man dies, he has been shot through the heart. The method treated fairly, will not even, by itself, justify the universal proposition that if a man is shot through the heart, he dies. Of course, in such a case, we do not argue 'inductively' at all, but deductively from our knowledge of physiology and of firearms. To really test the method, therefore, we must put ourselves in the position of one who has no such knowledge, and who has never seen firearms or wounds inflicted by them. A person in such a position would not be justified in drawing a universal conclusion. The shot has caused death in *this* case; but is it due to the fact that it passed through the heart, or to some individual peculiarity of the man; and if the former would death always attend such a wound? The method gives no means of answering these questions. Mill, indeed, acknowledges this. Even whilst affirming that plurality of causes does not affect the Method of Difference, he says: "For if we have two "instances, *ABC* and *BC*, of which *BC* gives *bc*, and *A* being "added converts it into *abc*, it is certain that in this instance "at least, *A* was either the cause of *a* or an indispensable "portion of its cause, even though the cause which produces "it in other instances may be altogether different" (III., x., § 2). But this is to acknowledge that plurality of causes does affect the method, and that, consequently, it can never prove a general proposition.

Mill both denies and affirms that Plurality of Causes affects the Method of Difference.

Moreover, it cannot be granted that there is any rigidly accurate proof even in the individual case. The method—like all the others—assumes an independence of elements which Mill himself tells us is fictitious. What really happens when into *BC* a new element *A* is introduced is that *A* is some modification of *B*, and not an independent element. Thus, let *B* represent the course of a ship and *C* the rate of its movement. If then we have as a new element, *A*, a movement of the helm, this leads to an alteration of *B*. If as a consequence of this change the vessel runs on a rock, *a*, this is not the effect of *A*, but of *BC* as modified by a change causally connected with *A*. Or, again we may have *BC—bc* of such a character that *B* simply negatives the action of *C*; the introduction of *A* may then neutralize *B* and so allow the effect of *C* to be apparent. The new result *a* may, then, be the effect of *C*, and not that of *A* at all. For example let *B* be the closed tube of a barometer, *C* the pressure of the atmosphere, and *b* the height of mercury in *B*. If, now *A* represents the breaking off the top of *B*, then the introduction of this new antecedent leads to *a*, the fall of the mercury in *B*. But *a* is due to *C* which had hitherto been neutralized, so far as *b* was concerned, by *B*.

The methods all assume an unreal independence of elements,

Closely connected with this error of regarding elements of reality as independent, is that of neglecting the presence of counteracting causes. Mill tells us later that "All laws "of causation are liable to be . . . counteracted, and seem-"ingly frustrated, by coming into conflict with other laws, "the separate result of which is opposite to theirs, or more "or less inconsistent with it. And hence, with almost every "law, many instances in which it really is entirely fulfilled, "do not, at first sight, appear to be cases of its operation at "all" (III., x., § 5). But this admission vitiates Difference as well as all the other methods. For if we have *ABC—abc, BC—bc,* how can we be certain that because *a* is not apparent in the latter case, it is really absent?

and neglect the possibility of counteracting agencies.

But if the Method of Difference is thus invalid as a criterion of proof, the subordinate methods cannot be expected to be in better plight. Mill tells us plainly that it

The Canon cannot be fulfilled

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in the Joint Method,

is never possible to fulfil the condition of agreement in the presence and absence respectively of a single circumstance which the canon of the Joint Method demands (III., viii., § 5), and thus himself condemns it. And, like Agreement and Difference, and for the same reasons, it cannot prove a general proposition.

or in that of Residues,

So again with the Method of Residues. We "can never be "quite certain" that "*C* is the *only* antecedent to which the "residual phenomenon *c* can be referred" (Mill, III., viii., § 5), and this canon, therefore, is likewise acknowledged to be incapable of application as a method of proof. And again, even if this difficulty did not exist, no general conclusion could legitimately be drawn unless *A, B, C* were incapable of influencing each other.

or in that of Concomitant Variations.

The same thing holds with the Method of Concomitant Variations. Except on the unwarranted assumption of the absolute independence of the elements involved, the conclusion is only valid in the very case or cases which are examined. And if the method is applied to unanalysed facts, then no conclusion at all as to causation can legitimately be drawn; for it is always possible that the concomitant variations are simply casual coincidences.

Summary.

Our examination of the Methods has, then, led us to the conclusions, first, that they are not inductive, in Mill's sense, for they can only start from universals, and their mode of inference is deductive throughout; secondly, that not only are they not "the sole methods of proof," as Mill claims they are, but that in no case can they furnish a valid proof at all.

The methods suggest hypotheses;

The fact that the methods do not satisfy the high claims made for them does not, however, prove that they are worthless. It is true that they are not 'inductive,' in the empiricist sense, in which Mill uses the word, of enabling the enquirer to gather and prove universal laws by a simple comparison of particular facts; but they are inductive in the sense that they are based upon principles which are operative in inductive enquiry, though in every case they need more exact formulation. Mill himself gives the key

to their main function when he says the methods supply the first generalizations upon which such a conception of induction as that of Whewell can work (III., ix., § 6). In other words, the methods suggest hypotheses. This is apparent with the Method of Agreement, which is really nothing more than an uncertain argument from an enumerative induction in which an attempt is made to weigh the instances as well as to number them, an attempt which really ends in an argument from analogy (*see* § 149). The "Method of "Residues" we have already spoken of as a frequent origin of hypotheses, and all the examples which Mill quotes from Herschel show that this is its function. That it was so regarded by Herschel is evident from his speaking of the method as making apparent "a residual phenomenon *to be* "*explained*" (*Nat. Phil.*, § 158). The other methods also may suggest hypotheses, and Concomitant Variations generally does so.

and are, in principle, applicable to verification by perceptual analysis.

They are besides more or less imperfect expressions of the true methods of testing and verifying hypotheses by perceptual analysis. The Joint Method rests on the true principle of recognizing the value of negative instances, but, as a method applicable to observation only, it can never give absolute proof, though it can yield a very high degree of probability, so long as it is applied to circumstances in which the conditions are known with considerable fulness. The Method of Difference is based upon the principle which governs all experiment — that of varying one circumstance at a time; but it does not sufficiently recognize the force of the negative instance. Mill sometimes spoke as if two separate instances were necessary, in one of which *A* was present and in the other absent. In this case it is, of course, impossible to secure that the instances are alike in all but *A*. But in some of his examples the *A* is introduced into the *BC* and the two instances are two successive states of the same thing. This we have seen to be the essence of experiment. An amended Canon of the Method, which is free from the objections urged against Mill's statement, is given by Mr. Laurie: "If,

Mr. Laurie's more exact formulation of the Method of Difference.

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"into circumstances found to be incapable of producing a "certain event, a new phenomenon or set of phenomena "be introduced, and the event in question occur, the new "phenomena or set of phenomena is the cause, or part of "the cause, of the event. If the removal of any given "antecedent makes no difference in the occurrence of the "event, that antecedent is irrelevant, while antecedents "which cannot be eliminated without eliminating the event "are causal. And the Universal Law of Causation compels "the inference that, if these conditions be repeated, the effect "will also occur" (*Mind*, N.S., vol. ii., pp. 326-7). Here "antecedent" and "consequent" have no necessary reference to time sequence; they are equivalent to "determining" and "determined." But Mr. Laurie goes on to point out that: "To establish a strictly invariable connexion between "a consequent and definite antecedents, the Method of 'Difference must be supplemented. . . . Such an extension "of the Method of Difference . . . may be expressed in the "following Canon: When, by the Method of Difference, "we have established a causal law connecting certain con- "ditions with the production of a phenomenon, and when, "further, we have failed to discover any case in which the "phenomenon occurs without those conditions, there is a "probability, increasing with the extent and variety of our "negative instances, that the phenomenon can be produced "in no other way" (*ibid.*, pp. 332-3). In other words, the force of negative instances must be recognized in the attempt to establish not only the proposition *If S is a it is x,* but also *If S is not a it is not x,* the latter being equivalent to the reciprocal of the former. In so far as the Method of Concomitant Variations is a modification of that of Difference, thus amended and supplemented, *i.e.* in so far as it is a mode of quantitative experiment, it also becomes a method of proof.

Mill himself states the true function of the methods.

The only true function of the methods is, indeed, given by Mill himself, when, in speaking of cases in which there is an "interference of causes with one another," he says: "The "instrument of Deduction alone is adequate to unravel the

"complexities proceeding from this source; and the four "methods have little more in their power than to supply "premises for, and a verification of, our deductions" (III., x., § 3). As Mill also tells us that "all laws of causa- "tion are liable to be in this manner counteracted, and seem- "ingly frustrated" (*ibid.*, § 5), it is evident that he himself attaches very different values to the methods in different parts of his work; as we have said before, he is constantly vacillating between two views. This latter statement of the function of the methods may be accepted, though, as we have seen, even as modes of verification they need more exact and accurate formulation.

CHAPTER VI.

QUANTITATIVE DETERMINATION.

156. Measurement.

Laws of Nature can only be stated with perfect exactness in quantitative form.

We considered in the last chapter how the existence of a necessary relation between phenomena can be established. But all perceptible facts are measurable, and, consequently, a relation between them is stated with perfect definiteness and exactness only when it is expressed in quantitative form. We must not only be able to say generally that *If S is A it is X*, but that for every definite value a_1 of A, there is a corresponding definite value of x_1 of X. Thus, as Mach says, "The "laws of nature are equations between the measurable "elements of phenomena" (*Sc. of Mechanics*, p. 502). No doubt the attainment of precise mathematical statement of law is only possible in the most advanced branches of science. Every branch of knowledge is at first merely qualitative, or at best very roughly quantitative. The ancient Chaldean astronomers, for example, were satisfied to record an eclipse to the nearest hour, whilst now the time is fixed to a very small fraction of a second. As the power of exact measurement is increased, science advances; and thus the invention of a new and more exact instrument of measurement has, as Jevons remarks, "usually marked, if it has not made, an epoch" in the branch of science to which it is applicable. This dependence of the advance of science on progress in the power of measurement is forcibly illustrated by De Morgan. "Had "it not been," he remarks, "for the simple contrivance of "the balance, which we are well assured (how, it matters not "here), enables us to poise equal weights against one another, "that is, to detect equality and inequality, and thence to

Science advances with the power of exact measurement.

"ascertain how many times the greater contains the less, we "might not to this day have had much clearer ideas on the "subject of weight, as a magnitude, than we have on those "of talent, prudence, or self-denial, looked at in the same "light" (*Formal Logic*, p. 175). Indeed, as scientific thought is exact thought, physical science cannot strictly be said to exist except in a quantitative form. As Herschel tells us: "Numerical precision . . . is the very soul of science; and "its attainment affords the only criterion, or at least the "best, of the truth of theories, and the correctness of ex-"periments. . . . Indeed, it is a character of all the higher "laws of nature to assume the form of precise *quantitative* "statement" (*Disc. on Nat. Phil.*, §§ 115-6).

Every law is abstract and hypothetical,

The laws of nature are, however, expressions of the relations between phenomena. Now, every concrete fact is complex; that is to say, it is due to the co-operation of several conditions, it is the embodiment of a plurality of relations [*cf.* § 146 (i.)]. Its whole nature, therefore, can never be expressed by one law, for each law is the statement of only one of these relations. Hence, each law is highly abstract, and is hypothetical in essence, even when expressed in a categorical form, as it states not what does happen, but what would and must happen under certain defined conditions. But it is frequently impossible to secure these conditions pure, *i.e.* unmixed with other and interfering conditions. For the relations which meet in a concrete fact are not independent, and they frequently modify and wholly or partially counteract each other. Thus, it happens, that many quantitative laws—as, *e.g.* the law of inertia—are never exactly realized in fact. Hence, it appears evident that mere observation of facts will never enable us to see in them those laws which are only imperfectly fulfilled, and that the procedure is here, as it has been throughout, by hypothesis. Such quantitative relations are conceived as will, in their mutual action, explain the concrete phenomena we observe.

and is liable to total or partial counteraction.

Facts are only known as observed

This becomes more evident, however, when we remember that facts are known to us only as they are observed by us, and that all observation is determined not only by the fact

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and all observation is liable to error.

but by the observer [*cf.* § 153 (i.)]. And all observation, whether made with or without instruments, rests at bottom upon the accurate interpretation of sense impressions; for instruments and other artificial means of improving observation can do nothing but bring the observed object, by breaking it up, magnifying it, and so on, within that range of moderate quantities which can be gauged by our senses. But how erroneous our interpretation of sense impressions may be is obvious to anyone who considers, for instance, how much larger the moon appears to us when it is just above the horizon than when it is in the zenith, or again how the shape or size of the same object appears to vary with its distance from, and relative position to, the observer. It is true we never attempt to estimate absolute, but only relative, magnitude; in other words, all measurement is comparison with a standard unit. What character of unit should be employed for the measurement of any kind of quantity is a matter of the greatest practical importance, but it is one which concerns the particular branches of knowledge, not general logical theory. The recognition of this comparative character of measurement, however, makes clear that its accuracy is relative to our power to distinguish differences by our senses. And this power is limited, even with the most delicate instruments. "Few measurements of "any kind," says Jevons, "are exact to more than six signifi-"cant figures, but it is seldom that such accuracy can be "hoped for" (*Pr. of Sc.*, p. 303). When, then, it is said that two magnitudes are equal, all that is meant is that our most exact instruments fail to make apparent any difference between them; but it does not follow that with more delicate instruments some difference would not appear.

All measurement is relative to the power of the senses to distinguish differences in magnitude,

and is never exactly accurate.

But further, the actual correctness of measurements even within this limit of possible accuracy cannot be depended on. This is evident from the fact that two careful measurements of the same magnitude never exactly agree, and, therefore, one or both are erroneous. Indeed, "we may," as Jevons says, "look upon the existence of error in all measurements "as the normal state of things" (*op. cit.*, p. 357). Now

"error" means a discrepancy between the magnitude given by actual measurement, and that estimated as the application of the law to this particular case. It might seem, at first sight, that the law was, therefore, shown to need modification and restatement. But this is not necessarily the case. If, as the experiment is repeated more and more carefully, the results approximate more and more to the law, then the law is confirmed: exact coincidence must never be expected. For instance, a student "learns that two pints of steam at a "temperature of 150° Centigrade will always make two pints "of hydrogen and one pint of oxygen at the same tempera-"ture, all of them being pressed as much as the atmosphere "is pressed. If he makes the experiment and gets rather "more or less than a pint of oxygen, is the law disproved? "No; the steam was impure, or there was some mistake. "Myriads of analyses attest the law of combining volumes; "the more carefully they are made, the more nearly they "coincide with it." Or again, "The place of a planet at a "given time is calculated by the law of gravitation; if it is "half a second wrong, the fault is in the instrument, the "observer, the clock, or the law; now, the more observations "are made, the more of this fault is brought home to the "instrument, the observer, and the clock" (Clifford, *Lectures and Essays*, pp. 91-2). It thus follows, that our laws are more exact than our observations and experiments can ever be, and that, consequently, they cannot be derived from mere comparison of the results of these observations, but are hypotheses which are more or less exactly verified by those experiments. Indeed, even were our measurements exact, yet they are only of isolated quantities, whilst the law gives the general formula of relation which claims to be applicable to all other degrees of magnitude of the same phenomenon. The law is thus seen to be a result of the synthesizing activity of mind, an activity which leads us to think of all nature as governed by universal and definite laws.

Laws are more exact than experiments, and are consequently not derived from them.

As, then, every measurement is more or less inaccurate, the question next arises as to whether, and how far, such error can be explained. In some cases it can; allowance

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Some errors can be allowed for as their causes, character, and amount are known,

may be made for the influence of modifying conditions when they are known to exist; *e.g.* for friction in any experiment dealing with the law of inertia; for temperature in experiments with the pendulum, or in measuring with metal rods. In other cases, there is a known and constant source of error in the instrument by means of which the observation is made—*e.g.* a clock may be known to be a second too fast or too slow. Again, there is with all observers a "personal equation" which is practically constant, as it is always in the same direction and of the same average amount; that is, one observer will always note the time of an observation a little earlier than another. Thus, "the difference between "the judgment of observers at the Greenwich Observatory "usually varies from $\frac{1}{100}$ to $\frac{1}{3}$ of a second, and remains pretty "constant for the same observers" (Jevons, *op. cit.*, pp. 347-8). The average amount of this personal error may then be calculated and allowed for.

but this is not the case with other errors,

But there are other errors which cannot be explained. They are due to causes which are unknown, or whose influence is so inconsistent that it cannot be calculated. They may result from the operation of some law not known to the observer, or to the imperfection of man's powers of observation and of his instruments. On this we will quote an excellent passage from De Morgan's *Essay on Probability.* He says: "To note a measurable phenomenon without "any error at all, would require sight and touch by "which every magnitude, however small, could be perceived "and correctly estimated. Such senses belong to no one, and "the degree of approach towards perfection not only varies "with the observer, but is different at different times with "the same observer. Many errors to which instruments are "subject ought in strictness to be classed under the first "head; if, for instance, an astronomical circle gradually "change its form, or undergo daily expansion and contrac- "tion by variations of temperature, the diversity of results "which such a piece of brass will show are certainly subject "to laws, and might be predicted, if we possessed sufficient "knowledge of the constitution of the metal, and the laws

"which regulate the effect of pressure, temperature, moisture, "etc., upon it. But so long as such laws are unknown, and the "variations do not follow any distinguishable rule, their effect "upon general results differs in nothing perceptible from that "of the observer's own errors, with which they are mixed up "in the particular results of observation" (pp. 130-1). When, therefore, allowance has been made for all known sources of error, there yet remain discrepancies whose causes, character, and magnitude are unknown. To eliminate these we must—as in all cases where the conditions of a phenomenon are unknown [*cf.* § 146 (i.)]—have recourse to the theory of probability for guidance. The logical nature of this theory must, then, next engage our attention.

and the elimination of these rests on the theory of probability.

157. Probability.

(i.) Basis of the Theory.

Chance is not objective.

When, in any particular case, we do not know what conditions are operative, we cannot tell, on the one hand, what result will appear, nor, on the other, can we say positively what conditions have produced a certain given event. In such cases we are accustomed to speak of the occurrence as due to chance. But our whole conception of the unity of nature forbids the idea that any element of reality can be really casual. Every detail is, in the strictest sense, necessary, and determined absolutely by conditions—all is causal, nothing is casual. Were our knowledge complete, then, the idea of chance would disappear; it is due solely to the imperfection of that knowledge. This imperfection is, of course, greater in some cases than in others; it may affect the event as a whole, or it may only affect some particular aspect of it. But, even with imperfect knowledge, we are often called upon to come to a decision or to act. The question then arises as to what we ought rationally to expect.

Probability is the measure of rational expectation,

The estimation of such rational expectation is the province of the Theory of Probability. Probability is, thus, seen to be subjective in the sense that, when we say that the probability that an event will happen in a certain way is $1/n$, what

we mean is that the relative amount of knowledge and ignorance we possess as to the conditions of the event, justify that amount of expectation. The event itself will happen in some one definite way, exactly determined by causation; the probability does not determine that, but only our subjective expectation of it. Indeed, without the assumption of the necessity of the actual event, there would be no basis for the calculation of probability. The very foundation of the theory is that possibilities can be limited to a definite finite number. We know, in other words, that the general relation *If S is A it is X* holds; but we do not know what particular form—x_1 *or* x_2 *or* . . . x_n—X will take because we do not know in what exact form—a_1 *or* a_2 *or* . . . a_n—the general condition A will be realized, though we are bound to assume that every definite form a_n necessitates one perfectly definite form x_n.

and presupposes objective necessity.

The logical basis of calculations of probability is a disjunctive judgment, whose alternative predicates are: 1. exhaustive; 2. definite; 3. exclusive; 4. equal in value.

It is from this combination of knowledge and ignorance that the calculation of probability starts. Its logical basis is thus seen to be the disjunctive judgment. But certain conditions must be fulfilled by the alternative specifications which form its predicate. They must first, as has been already said, be exhaustive of the whole range of possibility. Secondly, they must be definite. and therefore, in agreement with the principle of identity. Thirdly, they must be mutually exclusive. As we saw in our discussion of the disjunctive proposition, the form of the proposition does not guarantee this exclusiveness though the nature of the alternatives often does (*see* § 79[1]). But where there is any doubt on the matter, the disjunction may always be so written as to make all the alternatives *formally* exclusive—thus *S is either* $m\bar{n}$ *or* $\bar{m}n$ *or* mn expresses, with formally exclusive predicates, the disjunctive proposition *S is either m or n*. Lastly, the alternatives must be of equal value, *i.e.*, if they are given—as they always may be—as alternative consequents in a hypothetical proposition, those consequents must be equally likely. If the disjunctive proposition is given in categorical form, this condition postulates that the alternative predicates shall be either equal and co-ordinate specifica-

[1] First Edition, § 89.

tions of the common genus, or shall refer to equal extents of denotation. Thus, if an urn contains one black and six white balls, the disjunction that a ball drawn will be either white or black is doubtless correct; but it is not a basis for the calculation of probability, for black and white are not specifications of equal value of the genus ball. Or, stating the proposition in hypothetical form—If a ball is drawn, it is either white or black—the consequents are not equally likely, owing to the preponderance in number of the white balls. And generally, this condition of equivalence can never be fulfilled when one of the alternatives simply negatives the other, owing to the indefinite range of reference of the negative term [*see* § 29 (i.)[1]]. Indeed, such a disjunction would not fulfil the second condition given above, which requires that each alternative shall be definite.

The assumption that the alternatives are equally likely is justified by ignorance of the exact conditions which determine the event,

But it is necessary to enquire into the justification for regarding the alternatives as equally likely. This justification must be found in our ignorance as to what precise conditions will be operative. For example, if a penny is tossed, it will fall with either head or tail uppermost. Now, which will be uppermost in any particular throw will be exactly determined by such conditions as the position of the coin at starting, how it is grasped in the fingers, the force and direction of the twist, etc. But what special form these conditions will take we are totally ignorant. We know that if S (the coin) is tossed (A), it will fall with one side uppermost (X); but as we do not know the form a_n which A will take, we do not know whether X will appear as x_1 (heads up) or x_2 (tails up). There is, thus, in every such calculation a basis of knowledge. We know the coin will lie on one of its sides and not on its edge; but we have no reason to expect one side rather than the other to be uppermost, that is, we have no reason to believe the chances to be unequal. But further, we also know that in a long series of throws the sides which come upwards will succeed each other very irregularly, and yet with an approximation to equality in frequency. We have thus, in addition, objective and positive reason to believe that the chances for head and tail are at any rate approxi-

and the assumption is approximately verified by experience,

[1] First Edition, § 35 (i.).

mately equal. Experience thus tends to verify the hypothesis of equal probability to which we are led on the subjective ground that we know no reason to expect one result rather than the other. For when this is the case, we expect that these unknown conditions will in the long run make as often for one result as for the other, and, therefore, that the possible results will be realized with equal frequency. Of course this can never be exactly verified, as that would involve the examination of an infinite series of occurrences, which is impossible. To stop in a series of observations—*e.g.* of the throws of a penny—when the possible events have all happened an equal number of times, would obviously be to beg the question; for a continuance of the series would, in all likelihood, show a divergence from equality. But as, in a long series, the approximation to equality is always fairly close, experience may be said to give as full confirmation of the theory as the case permits. The calculation of probability is, nevertheless, not dependent upon the actual experience of any series. It is possible, not only when we have grounds for believing the chances to be equal, but when we have no reason for assuming them to be unequal. In fact, our whole data is knowledge of the number of equivalent possible cases, and the absolute absence of any ground for preferring one rather than the others. We may calculate the probability just as well when only one of the possible cases can be actually realized—*e.g. one* throw of a penny—as when we are dealing with a series in which all the alternatives will be actually realized an approximately equal number of times.

though actual experience is not necessary to its formation.

It is said sometimes that the idea of equally likely alternatives is based upon equality of belief in their occurrence. This is to make the question too entirely and individually subjective. The belief of any individual is influenced by many conditions, some of which are generally more or less irrational, and independent of any valid justification. Thus, belief in the possible occurrence of any event would vary with different individuals. But the probability does not vary with it. It is not, therefore, actual belief, but what ought to be believed, that is defined by probability. And what ought

Calculation of probability is not based on belief.

to be believed is but another name for rational expectation, that is, expectation based upon the proportion of available knowledge to ignorance. Probability is, therefore, a measure of knowledge, and so defines what ought to be believed, instead of being based upon what actually is believed.

The degree of probability is not dependent on time.

From what has been said, it is evident that the amount of probability is not affected by considerations of time. Whether the calculation refers to an event in the past or in the future, or to the truth of a universal proposition, is immaterial; it is in every case based on the same principles. No doubt most illustrations of the doctrine are drawn from games of chance, as such games exemplify the conditions in the simplest way, and these have generally a reference to future expectation. But our knowledge and ignorance may be combined with reference to a past event in the same way as to one in the future, and we must calculate the probability of the reality of that event on the same principles. Thus, the theory of probability is applicable to the credibility of testimony, as well as to the prediction of a future occurrence. No doubt, when an event has occurred, we can frequently get more exact knowledge of it, and then, of course, the need for the calculation of its probability is, to that extent, removed; but so long as we do not know exactly what has happened, but only that one out of a certain number of alternatives has occurred, the appeal to probability remains. The distinction so frequently drawn between probability before and after the event is due partly to the neglect of this consideration and partly to failure to make the alternatives equal in value. Thus, for example, it is extremely improbable that a hand at whist should consist entirely of trumps. Yet the probability of this is no less than that of any other one definite hand. It is its interesting character which draws special attention to it, and causes us to recognize how enormous are the odds against it. This we do not recognize in the case of other hands. If, then, a person told us he had held a hand of thirteen trumps the previous evening, we should probably feel more hesitation in believing him than if he told us he

had held a hand consisting of certain definitely named cards. Yet the antecedent improbability would be no greater in the one case than in the other. Were the person, however, to claim that he had, previously to playing, written down the contents of a certain hand, and that he had been actually dealt that hand, we should probably hesitate to receive his statement just as much as if he told us he had been dealt thirteen trumps; for the previous defining of the hand would have made the odds against it as apparent as in the other case. When we hesitate to believe the statement of such coincidences it is because we feel that the odds against the occurrence were antecedently very great, and we balance that with the odds in favour of the credibility of the witness. If we do not doubt his credibility we receive the statement in spite of its antecedent improbability, for to assume that the extremely improbable is impossible is to fall into a dangerous fallacy. The way in which we use our terms in discussing such a case shows that probability and improbability are not opposed terms in the theory of probability; improbability only means a very low degree of probability. It is, however, very vague in its reference; so, in all calculations, the probability only is spoken of, and this may, of course, be very high or extremely low.

(ii.) Estimation of Probability.

A full exposition of probability belongs to mathematics.

The full exposition of the applications of the theory of probability would take us far beyond our province into the region of mathematics. We are here concerned only with the logical character of the theory, and we will pursue its developments only so far as is necessary to show how it is based upon various modes of combining disjunctive judgments, and deductive reasoning from those combinations. The primary difficulty always is to secure in each case a full statement of exhaustive, definite, exclusive, and equally likely, alternatives; and it is in the determination of this that the mathematical doctrine of combinations and permutations is mainly appealed to.

(a) **Probability of Simple Events.** If we have to consider simply one set of alternative and co-ordinate possibilities, our data may be expressed in the single disjunctive proposition *A is* a_1 *or* a_2 *or* . . . . a_n, where we have n alternatives fulfilling the four conditions enumerated above. As there is no reason for preferring any one of these alternatives, the probability of each is equal. But as the alternatives are assumed to exhaust all possibilities, it is obvious that their sum must be equivalent to certainty, for we know A will be realized and are only ignorant as to which of the forms a_1 a_2 . . . a_n that realization will take. This certainty is most appropriately represented by unity. The probability of each of the n co-ordinate alternatives a_1 a_2 . . . a_n is, therefore, represented by the fraction $1/n$. On the same lines, impossibility, *i.e.* the absence of any chance of realizing A in any form, would be represented by o/n, *i.e.* by zero.

The probability of each of n correctly stated alternatives is $1/n$.

Now, if there are n possibilities, the chances against the realization of any particular one are obviously $n-1$; for the sum of the chances for and against must be the total number of chances as $n/n=1$. This is expressed by saying the *odds* against any one particular case are $n-1$ to 1. Now, if a number of similar but distinct possibilities are massed together as one alternative, that alternative must be given its true value. For example, if there are n balls in an urn, of which one is white and the rest black, then the chance of drawing black is $(n-1)$ times that of drawing white, and the probabilities are not equal, but are represented by $1/n$ for white, and $(n-1)/n$ for black.

Odds = chances against.

(b) **Probability of Compound Events.** By a compound event is meant one in which two or more simple events occur in connexion with each other. The logical basis of the probability of such an event will, therefore, be found in the combination of two or more disjunctive judgments, fulfilling the conditions enumerated above. The greatest care must be taken in the statement of the alternatives to make them both exclusive and exhaustive.

The basis of the probability of compound events is a combination of disjunctive judgments.

(1) *Independent Events.* If we have two independent disjunctive judgments, *i.e.* judgments whose predicates are

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neither necessarily connected, nor necessarily incompatible, with each other—

A is either v or w
A is either x or y or z,

then by combining them we get the judgment

A is either vx or vy or vz or wx or wy or wz.

Similarly, if the disjunctive propositions are also hypothetical—

If S is a, it is either v or w
If S is b, it is either x or y or z,

then the combination will give

If S is both a and b, it is either vx or vy or vz or wx or wy or wz.

The probability of a compound event is the product of the probabilities of the simple events which compose it.

In each case the number of alternatives in the joint proposition is the product of the number of those in the single propositions, and as a result, the probability of the joint event is equal to the product of the probabilities of the simple events which it embraces. Thus, in the above symbolic examples, the probability of *v* is $1/2$, and that of *x* is $1/3$, whilst the probability of *vx* is $1/6$. Generally, if the probability of an event, *a*, is $1/m$, and the probability of another event, *b*, is $1/n$, then the probability that *a* and *b* will both occur is $1/mn$. But, as the direct basis of the joint probability is a single disjunctive proposition, the distinction between single and compound events is seen to be somewhat arbitrary, and to be nothing more than a distinction between two ways of regarding an occurrence.

Example from the ballot.

As an example we will take the following simple case. Let there be two urns each containing three balls, of which two are white and one is black. What is the probability of drawing a black ball from each? It might seem at first sight as if the two drawings give only four alternatives, viz. *ww, wb, bw, bb,* where *w* and *b* denote white and black respectively. But this is to neglect the fact that there are twice as many white as there are black balls; in other words,

the alternatives are not of equal value. If we symbolize the contents of the first box as w_1 w_2 b_1, and that of the second as w_3, w_4, b_2 we then have the separate disjunctives, in which this objection is removed—

$$\begin{array}{l} A \textit{ is either } w_1 \textit{ or } w_2 \textit{ or } b_1 \\ B \textit{ is either } w_3 \textit{ or } w_4 \textit{ or } b_2 \end{array}$$

Then the combined disjunctive is

$A\ B$ *is either* w_1w_3 *or* w_1w_4 *or* w_1b_2 *or* w_2w_3 *or* w_2w_4 *or* w_2b_2 *or* b_1w_3 *or* b_1w_4 *or* b_1b_2

where the number of alternatives is seen to be nine, only one of which is *bb*, and, consequently, the probability of drawing two black balls is *1/9*, which is in accordance with the general rule given above. If we examine the other alternatives in the final proposition we see that there are four cases of two white balls ; therefore, the probability of drawing a white ball both times is *4/9*. This is also in accordance with the general formula ; for the probability of white in each case separately is *2/3*, and, therefore, the probability of white in both cases is $2/3 \times 2/3 = 4/9$. Lastly, as the probability of white is in each case *2/3*, and that of black is *1/3*, so *2/9* is the probability both that the drawings will give first white and then black, and that they will yield first black and then white ; this is shown to be true by the fact that the final proposition yields two cases in which white is followed by black and two cases in which black is followed by white. The sum of these probabilities is, of course, unity, as one of the alternatives must occur. The general principle is thus illustrated at every point.

Example from throwing dice.

A similar example is found in the throwing of dice. If one die is thrown the probability that it will fall with the side bearing six pips uppermost is *1/6*, as the die has six sides, and the probability of being uppermost is equal for them all. If a second die is thrown, the probability that in that throw six will be uppermost is also *1/6 ;* consequently, the probability that six will be thrown in each of two throws is $1/6 \times 1/6 = 1/36$. In this case, it is obviously immaterial

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whether two dice are thrown simultaneously or whether the same die is thrown twice successively. So in the former example, whether the two drawings are made from two urns each of which contains two white balls and one black one, or successively from one of those urns, makes no difference, so long as the constitution of the one urn is kept constant by returning after the first drawing the ball which had been drawn. This again illustrates the fact that the distinction between simple and compound events is an artificial one.

The same principle applies to cases in which the second event depends on the first.

(2) *Dependent Events.* This leads us to the case in which the events are not independent. This, however, in no way differs in principle from the one we have just considered, though still greater care is required in stating the alternatives. The case, stated generally, is when the second event depends in some way on the first, so that the probability of the realization of the second is modified by the occurrence or non-occurrence of the first. Perhaps, this will be best understood if we begin with the examination of an example.

Example from the ballot.

Starting, then, with the same supposition as before, of an urn containing three balls, two of which are white and one is black, we will ask what is the probability of drawing white twice in succession, if the ball extracted at the first drawing is not returned? In this case the constitution of the box is obviously different at the second drawing from what it was at the first, and the probability of the second drawing yielding white depends upon the result of the first drawing. If black is drawn the first time, for example, nothing but white balls remain in the urn, and the second drawing must, therefore, certainly yield white. Whilst, if white is drawn the first time, there remain only one white and one black ball in the urn, and the chance of drawing white at the second draw is, in that case, *1/2*. This last case is the only one which concerns us; as in the former, the first drawing yielding black obviously makes it impossible to get white at both the first two drawings. The two consecutive drawings from the same urn are, therefore, identical with a simultaneous drawing from two urns, one of which contains two white balls and one black ball. and the other contains

one white and one black ball. The disjunctions may, therefore be written—

A *is either* w_1 *or* w_2 *or* b_1
B *is either* w_3 *or* b_2

where w_3 represents in the case of the single urn whichever of the white balls, w_1 *or* w_2, is not extracted at the first drawing. Combining these we have

$A\ B$ *is either* $w_1 w_3$ *or* $w_1 b_2$ *or* $w_2 w_3$ *or* $w_2 b_2$ *or* $b_1 w_3$ *or* $b_1 b_2$

and there are seen to be six alternatives, including two cases in which two white balls are drawn. The probability of this drawing is, therefore $2/6=1/3$. Similarly the probability of drawing two black balls is seen to be $1/6$. The same result is reached if we reason in this way—The chance of drawing white the first time is $2/3$. But only if white is drawn the first time is the second drawing proceeded with. Hence the probability of the second drawing taking place at all is $2/3$, and the probability of that drawing yielding white is $1/2$, as it is made when the urn contains only one white and one black ball. Hence, the probability of a second white ball being drawn is $2/3 \times 1/2 = 1/3$.

Example from tossing a coin

A similar example is the enquiry into the probability that if a penny is tossed up three times, head will fall uppermost each time. The probability that head will appear at the first throw is $1/2$, as head and tail are the only two alternatives. The probability of the second toss taking place is, therefore, $1/2$, and the probability of its yielding head is $1/2$. The probability of two consecutive heads is therefore $1/4$. But only when this is secured does the third throw take place; the probability of there being a third throw is, therefore $1/4$, and the probability that it will give heads is $1/2$. Therefore, the probability of three consecutive heads is $1/8$. The original disjunctions are—

A *is either* h_1 *or* t_1
B *is either* h_2 *or* t_2
C *is either* h_3 *or* t_3

and the combination is

A B C is either $h_1h_2h_3$ *or* $h_1h_2t_3$ *or* $h_1t_2h_3$ *or* $h_1t_2t_3$ *or* $t_1h_2h_3$ *or* $t_1h_2t_3$ *or* $t_1t_2h_3$ *or* $t_1t_2t_3$

where it is seen that only one case out of eight secures three heads.

Thus, the probability of the conjoined event is seen to be in this, as in the former case, the product of the probabilities of the separate events, and this is found to hold both when one event is contingent upon the occurrence of another, and when the separate events are independent. The results are different in the two cases, because in the former the number of possibilities of the second event is changed by the occurrence of the first event. But the general formula given in the last paragraph—If the probability of *A* is *1/m* and the probability of *B* is *1/n*, then the probability *A B* is *1/mn*—holds in every case.

If several conditions have the same consequent, the probability of that consequent is the sum of the probabilities of those conditions.

(3) *Events which can happen in a Plurality of Ways.* When we have obtained a correct statement of alternatives fulfilling the conditions of exhaustiveness, definiteness, exclusiveness and equal likelihood, we sometimes find that several of them are followed by the same consequent. For example

S is either a or b or c,
If S is a it is x,
If S is b it is x,
If S is c it is y.

Here it is obvious that the result is that *S is either x or y*, but that the probability of *x* is twice as great as that of *y*, as *x* will occur both when *a* is realized and when *b* is realized, and the probability of both *a* and *b* is seen, from the original proposition, to be *1/3*. Hence, the probability of *x* is *2/3*. If in the original disjunction the probability of *a* had been *1/2*—*i.e.* if the proposition when accurately stated ran *S is either a or a or b or c,* then the probability of *x* would, on the conditions given in the hypothetical propositions above, be *3/4*. Thus to put the matter generally: When several distinct and

exclusive conditions have the same consequence, the probability of that consequence is the sum of the probabilities of those conditions. This, it will be seen, is not a new principle, but a simple deduction from the conditions which a set of alternatives must satisfy in order to form a basis for the calculation of probability.

Example from tossing a coin.

As a simple example, we will ask what is the probability that head will come uppermost in one or other of two throws of a penny. Here there are four alternatives in the original disjunctive proposition. The throws are either *hh or ht or th or tt*, and any one of three of these secures a head. Of course in the first case, when head is once secured the second throw would not be made; but the case must not be omitted, as there are two possible sequences in which head comes first. This is seen quite clearly when it is remembered that two consecutive throws of the same coin and the simultaneous tossing of two coins are identical as a basis for computing probability. We reach the same result by a line of reasoning similar to that we appealed to in the last case [*see* (2)]. The chance of getting head at each toss is $1/2$; but the second toss is contingent on the failure of the first to give heads; the probability of the toss itself is, therefore, $1/2$, and consequently the probability of its giving heads is $1/2 \times 1/2 = 1/4$. Hence, the total probability of getting heads in one or other of two tosses is $1/2 + 1/4 = 3/4$.

Example from throwing dice.

For another illustration we will take the probability that a throw of two dice will yield a certain definite number of pips, say seven, irrespective of how that number is divided between the faces of the two dice. Our original disjunction here consists of thirty-six alternatives; each of which is a condition fulfilled in one throw, and yielding a particular consequence. Upon examining them, we find that the number seven results from six of these alternatives, as it may be composed of $4+3$, $5+2$, or $6+1$, and each of these may occur in two ways, according as the larger number appears on the first or the second die. The probalility of seven being thrown is, therefore, $6/36 = 1/6$. Similarly, the probability that each die will throw the same number of pips is

1/6, as there are obviously six possible cases in which a pair of numbers can be shown.

'Inverse' Probability is governed by the same principles as 'Direct' Probability.

(c) Probability of Alternative Conditions. We will now consider what is often called "inverse" probability, which stands in the same relation to the direct probability we have been hitherto dealing with as induction does to deduction. As the same principles of reasoning govern both those methods of inference, so here we shall find no new principles. The problem is—given that a certain event has happened, to which of a certain number of alternative causes or essential conditions is it probably due, each being conceived possible? This is the problem that has to be solved in endeavouring to reach the true magnitude of a phenomenon from more or less inaccurate measurements. The problem, obviously, presents more practical difficulties in its solution than do those we have already dealt with, as we have often no certainty that we have correctly stated the alternatives. But the principle on which we proceed is the simple one that the condition from which the actual event would most probably follow is most probably the condition from which it actually did follow. We have, then, to take account of two probabilities —(1) that the assumed condition was actually existent, and (2) that the event would follow from it if it were present; the probability of the condition is then the product of these two probabilities. This is seen to be only the inverse mode of stating the direct probability of a compound event.

The probability of the hypothesis must be compounded with the probability that the event would follow if that hypothesis were fulfilled.

Example from the ballot.

To take a simple example. We will suppose that an urn is known to contain three balls but we are ignorant of their colour, and that after each drawing the ball drawn is returned to the urn. If one white ball is drawn we have no reason to conclude anything about the possibility of drawing or not drawing a ball of another colour. If the next drawing gives a black ball we have two alternatives, but we are not certain that they are exhaustive of all the possibilities. If, however, we continue drawing and get nothing but white and black balls, the probability that there are no balls of any other colour present becomes rapidly stronger. If, for instance, the urn contains one, and only one, red ball, the probability that that

ball would not be drawn at any one drawing is $2/3$. The probability, therefore, that it would not emerge in four drawings is only $2/3 \times 2/3 \times 2/3 \times 2/3 = 16/81$, whilst that it should not appear in eight drawings is only $2^8/3^8$ which is less than $1/25$, and with every drawing this probability decreases. On the other hand, if the urn contains only white and black balls, the result obtained would be a necessity. This assumption is, consequently, by far the more likely.

Second example from the ballot.

As a slightly more complex example, we will assume it known that the urn contains three balls which are known to be either white or black, and we will estimate the probability of the various possible proportions of white and black balls in the urn. Before the drawings commence there are four possible alternatives—*www*, *wwb*, *wbb*, *bbb*. The probability of each alternative is, therefore $1/4$, and that of any one particular ball is $1/3 \times 1/4 = 1/12$. The case is, indeed, the same as having twelve balls—six white and six black—to draw from. The first drawing, however, giving a white ball, the alternatives are reduced to three, *bbb* being put out of court. We then consider the relative probability of the remaining alternatives. As there are six ways in which *w* can be drawn, the probability for each individual white ball is $1/6$. The probability, therefore, that the white ball actually drawn came from *www* is $3/6 = 1/2$; that it came from *wwb* is $2/6 = 1/3$, and that it came from *wbb* is $1/6$. The most probable alternative, therefore, at the end of the first drawing is *www*. If, however, the second drawing gives a black ball, *www* is proved to be impossible, and the alternatives are reduced to *wwb*, and *wbb*. And so far, there is no reason to prefer one to the other. If, however, the next drawing is *w*, so that in three drawings *w* appears twice and *b* only once, the alternative *wwb* is the more probable. For assuming *wwb* to be real, the probability of *w* is at each drawing $2/3$ and that of *b* is $1/3$. Hence, the probability of getting a single combination giving *w* twice and *b* once is $2/3 \times 2/3 \times 1/3 = 4/27$. But this combination may be drawn in three different orders, viz., *wwb*, *wbw*, *bww*. Therefore the total probability of getting one of these three orders is the sum of them all, and as each

is $4/27$ the sum is $3(4/27)=4/9$. On the other hand, on the supposition of *wbb*, the probability of getting *w* twice and *b* once is $2/3\times 1/3\times 1/3=2/27$. This may also happen in three ways, and therefore, its total probability is $3(2/27)=2/9$. Thus the odds in favour of the first hypothesis are *2* to *1*, and its probability is therefore $2/3$.

Example from the credibility of testimony.

As another example we will take a case of the probability of testimony. Suppose that two witnesses, the probability of whose accuracy is $3/4$ and $2/3$ respectively, agree in affirming the occurrence of an event which in itself is as likely to have happened as not to have happened; *i.e.* whose antecedent probability is $1/2$. What is the probability that the event really did happen? There are two possible and equally likely antecedents—that the event did happen, and that it did not happen. On the hypothesis that it did happen the probability of both witnesses stating that it did is the probability of their both telling the truth, *i.e.* $3/4\times 2/3=6/12$. On the hypothesis that it did not happen, the probability that both witnesses should assert that it did is that of their both speaking falsely, *i.e.* $1/4\times 1/3=1/12$. The odds are, therefore *6* to *1* that the event did happen; *i.e.* the probability that it really occurred is $6/7$.

Continued repetition of an event increases the probability that it will occur again.

(d) **Probability of the Recurrence of an Event.**[1] If we know nothing whatever of the conditions of an event, but only that it has occurred, we may estimate the probability that it will occur again. At first sight it might seem that the chance of its occurring again under the same conditions is exactly equal to that of its not recurring; *i.e.*, that for every occurrence the probability would be $1/2$. But this would lead to the paradoxical result that continued repetition of an event gives us no grounds for expecting it to occur again, and so that even infinite uncontradicted experience of the occurrence would not increase our rational expectation that it should occur again. It is, however, a miscalculation, as it overlooks the fact that continued occurrence testifies to the persistence of the conditions which produce the event, and even though we do not know what those conditions are, yet as evidence of their existence increases, we are justified in

[1] The solution of this problem is that given by Lotze, *Logic*, § 282 (5).

expecting more and more strongly their continued existence. We must, therefore, take account of all the times the event has occurred, and in stating our original alternatives each of these must count as one. For instance, if an event has happened once, that is one reason for expecting its recurrence. But if we put that on one side for the moment, the chances for its occurrence again would be equal. There are, therefore, two reasons to expect its recurrence and only one for expecting it not to happen again. The odds for its recurrence are, consequently, *2* to *1* and the probability of the event happening again is *2/3*. So, generally, if an event has happened m times, that gives m alternatives in the original disjunction; the possibilities that it may occur again and that it may not do so, add *2* more. Consequently, the total number of alternatives is $m+2$ of which $m+1$ are favourable. The probability that the event will occur once more is, therefore, $(m+1)/(m+2)$. For instance, if the sun has risen daily for five thousand years the probability that it will do so once more is *1,826,214/1,826,215*. It is thus evident that with continued uncontradicted experience, the probability of a single repetition of the event rises very high indeed, and that it increases with the growth of such experience; the larger the value of m, the nearer the probability approaches to certainty. But this probability rests on the assumption that continued experience testifies to the persistence of the conditions which produce the event, and this assumption is itself only probable. The formula, therefore, measures the probability of the recurrence of the event only indirectly: it directly measures the probability of this probability.

Each occurrence is a reason for expecting repetition; therefore, if an event has occurred m times the probability that it will occur once more is $\frac{(m+1)}{(m+2)}$

It will be seen that this calculation of probability is the true basis of induction by simple enumeration. The formula shows that with wide and uncontradicted experience the probability that an empirical law which summarizes that experience will hold good in one more case is very high. But it also shows that extension of it beyond the realm of actual experience becomes increasingly uncertain with increase in the width of that extension. For, if the formula

This is the basis of induction by simple enumeration.

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The probability decreases with the number of unobserved cases to which the law is extended.

is written to show the probability that an event which has occurred m times will happen n times more, it becomes $(m+1)/(m+n+1)$ — for $m+2$ in the original formula $=m+1+1$, where 1 is the number of new cases, *i.e.* n— which obviously decreases in value as n is increased. Again, another modification of it shows how the actual experience of the failure of the event, weakens the probability of its recurrence. For if an event has occurred m times and failed to occur n times under circumstances where it might have been expected to happen, then there are already $m+n$ cases; the possibilities that it may or may not occur again add 2 more, and thus, the probability for its recurrence is $(m+1)/(m+n+2)$, which decreases as n increases. In this case, the extension to p more cases becomes still more hazardous, as its formula is $(m+1)/(m+n+p+1)$, where $(m+n)$ in the denominator corresponds to m in the original formula, as it expresses the total number of observed cases, and p corresponds to n as expressing the number of unobserved cases, the probability of whose occurrence it is desired to estimate.

158. Methods of determining Magnitude.

The true magnitude of a phenomenon can only be ascertained by inverse probability.

As different measurements of the same phenomenon vary, the ascertainment of the true magnitude is an inductive problem which can only be solved with a high degree of probable approximation. We have to decide what assumption as to the true magnitude will lead with the greatest probability to the values actually obtained, on certain hypotheses as to the character and mode of action of the unknown conditions to which the errors are due. We have the permanent cause which yields the true magnitude, and in addition an indefinite number of interfering conditions, more or less insignificant in amount, of whose number and influence we are ignorant. The question is—What assumptions are we justified in making with regard to these conditions; and, as a consequence of those assumptions, what practical methods are open to us of correcting our observations of magnitude?

(i.) The Method of Means.

If no reason to the contrary is known, it is most probable that the true magnitude lies between the extreme measurements.

When we have different results from several measurements of the same phenomenon, we know that all but one *must* be, and all *may* be, erroneous. As the number of measurements is increased, the probability of any one being exactly right gets less and less, and may, therefore, in our discussion, be left out of account. If then we have two different measurements, four alternatives are open to us—either both are too large, or both are too small, or either one is too large and the other one too small. If we are really entirely ignorant of any conditions likely to influence the result one way or the other, these alternatives will form a basis for the estimation of probability. If, however, the measurements are made with different instruments which have not the same degree of precision, or if one is made by a method more likely to yield accurate results than that used in the other case, we should prefer the measurement made under the conditions most likely to yield an accurate result. "Any method of measurement which we "know to avoid a source of error is far to be preferred to "others which trust to probabilities for the elimination of "the error. As Flamsteed says: 'One good instrument is of "as much worth as a hundred indifferent ones'" (Jevons, *Pr. of Sc.*, p. 391). But no methods or instruments give invariable and absolutely truthful results, though in all scientific work the methods are so well considered and so carefully employed, and the instruments are so delicate and precise, that we may regard the occurrence of any considerable error as a practical impossibility, and may assume that the larger a possible error is, the less is its chance of occurrence. If, then, we assume that the two measurements were made by methods and with instruments equally likely to give an accurate result, and if we are absolutely ignorant of any ground for prefering one measurement to the other—*i.e.*, for expecting one kind of error rather than another—we are justified in assuming that in each measurement it is equally probable that the result obtained is too large or too small. We have then two disjunctions A is l_1 or s_1, B is l_2 or s_2

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(where A, B symbolize the two measurements, l=too large and s=too small). Combining these, we get the disjunction A B is l_1l_2 or l_1s_2 or s_1l_2 or s_1s_2. From this it follows that the probability either that both measurements are too large or that both are too small is $1/4$, whilst the probability that one is too large and one too small is $1/2$. Thus the true magnitude most probably lies within the limits of A and B. The same result is obtained from the hypothesis that small errors are more probable than large, for it would follow from this that the most probable magnitude is that which gives the least sum of errors. But if the true magnitude is between A and B the sum of the errors is $A-B$, whilst if it exceeds A or is less than B by n, the sum of the errors of the two measurements is $A-B+2n$. As the number of measurements is increased, the probability that the true magnitude lies within the extremes rapidly increases. For example, if we have six measurements, the probability either that all of them are too large or that all of them are too small is only $1/64$, whilst the probability that the true magnitude lies between the extreme measurements is $62/64$. The odds are, therefore 31 to 1 in favour of that hypothesis.

The *Arithmetical Mean* is the most probable value when we are dealing with only one magnitude,

The question now arises as to *what* intermediate value is the most probable. The mathematical assumption is that the number of causes of error is infinitely large, whilst the influence of each is infinitely small, and that each is equally likely to be operative in increasing or in decreasing the result. Under these conditions, the arithmetical mean—*i.e.* $(a+b+ \cdots n)/n$—of a number of measurements of the same phenomenon is most probably the true magnitude; for the errors so caused will balance each other, the sum of the positive and that of the negative deviations being equal.

but the Geometrical Mean is more accurate when a condition acting according to the law of inverse squares is operative.

When, however, as happens in a few cases, some condition is known to be operative which varies as the square of the distance, the Geometrical Mean—*i.e.* $\sqrt{ab}$—gives the more accurate result. For example, if the true weight of an object is sought by weighing it successively in the two scales of an imperfect balance, as gravity is an operative condition, the true result will be the geometrical mean of the result,

though as, in small numbers, this differs but little from the arithmetical mean, the latter is generally taken as more easily calculated.

The Arithmetical Mean is of the most general application when one magnitude only is involved.

The arithmetical mean of the actual results of measurements is, then, the true magnitude—or the most probable approximation to that magnitude—in the great majority of cases when we are dealing with a single phenomenon. The errors in such measurements are related to each other as simply more or less in one fixed direction, and may be graphically represented by setting off on a straight line, on either side of a zero point which indicates the true magnitude—*i.e.* no error—divisions which are proportional to the positive and negative errors involved in the actual measurements.

(ii.) Method of Least Squares.

When a plurality of dependent magnitudes is involved, the most probable value is that which makes the sum of the squares of the errors the least possible. *This is the Method of Least Squares.*

But when we are dealing with compound magnitudes, in which two or more quantities are dependent on each other, the method of taking the arithmetical mean is no longer available. This case may be spatially represented by the deviations from the bull's eye of the shots on a target. The deviations are in all directions, but each can be resolved into a vertical deviation and a horizontal deviation. The graphic representation of each such resolution is a right-angled triangle, in which the actual deviation is the hypoteneuse; and the square on this equals the sum of the squares on the lines which represent its resolution into vertical and horizontal deviations. When the total deviations are measured, the sum of their squares—on the supposition that the sum of the horizontal variations on the one side cancel those on the other, and that the vertical deviations neutralize each other in like manner—is easily shown by a geometrical construction to be the least possible.

Geometrical illustration.

For if we call the true position P, and the actual positions obtained A, B, and C, then we may represent the deviations from P as a, b, c respectively. In decomposing these, we will assume that A is x above and m to the right of P, that B is y below and n to the right of P, and that C is z below and r to

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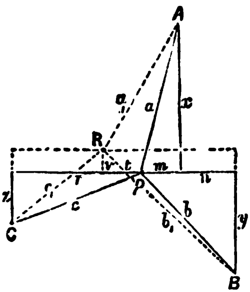
the left of P. Then $a^2=x^2+m^2$; $b^2=y^2+n^2$; $c^2=z^2+r^2$. If now we take any other position, R instead of P, we can express its relations to A, B, and C in similar terms. Suppose R is v above and t to the left of P. Then denoting the deviations of A, B, and C from R as a_1, b_1, c_1 respectively, we have $a_1^2=(x-v)^2+(m+t)^2$; $b_1^2=(y+v)^2+(n+t)^2$; $c_1^2=(z+v)^2+(r-t)^2$. Developing this we have $a_1^2+b_1^2+c_1^2=x^2+y^2+z^2+m^2+n^2+r^2+3(v^2+t^2)+2v(y+z-x)+2t(m+n-r)$. But, according to our hypothesis, if P is the true position $y+z-x=0$, and $m+n-r=0$, for they represent the vertical and horizontal deviations respectively. Thus $a^2+b^2+c^2$ is less than $a_1^2+b_1^2+c_1^2$ by $3(v^2+t^2)$.



The method of means is only a particular case of the method of least squares.

This is not offered as a proof of the method of least squares, but as an illustration of it. The proof, indeed, involves an appeal to mathematics which would be out of place here. The general statement of the method is: That magnitude is the most probable, whose assumption makes the sum of the squares of the errors of the actual measurements the least possible. It is seen by the above example to be an extension of the method of means, in that it indicates the most probable mean in cases which involve a plurality of arithmetical means. When we are dealing with only one magnitude the finding the arithmetical mean gives the same result as the application of the method of least squares, of which it is but a particular case. And it will be seen that this method shows the real agreement of the result obtained with the assumption that, as large errors are less probable than small, that result is most probable which gives the least possible sum of errors. If we take three measurements of a single phenomenon and obtain three values, the intermediate one will always give the least sum of simple errors, whether it is the arithmetical mean or not. For if a, b, c be the three results the sum of errors involved in assuming b to be the

true magnitude is—assuming them to be given in descending magnitude—$(a-b)+(b-c)=a-c$. But if the arithmetical average does not coincide with b but deviates from it by say n, then the sum of errors is $a-c+n$. For example, if our measurements are 15, 11, 10; then assuming 11 to be the true magnitude, the sum of errors is $4+1=5$; whilst 12, which is the arithmetical mean, gives $3+1+2=6$ as the sum of errors. And the same thing holds generally—the least sum of errors is obtained when the middle one of a series of measurements is taken as the true magnitude, if the series consists of an odd number of measurements; whilst if it consists of an even number, either of the two middle results, or a result intermediate between them, will yield the same, and that the least, sum of errors. But if we assume that the sum of the squares of the errors is to be the least possible, we find that this condition is fulfilled by the arithmetical mean alone. For example, on the supposition taken above, where the measurements were 15, 11, 10, if we assume 11 as the true magnitude, the sum of the squares of the errors is 17; but if we assume 12, the arithmetical mean, as the true magnitude the sum of the squares of the errors is only 14; and every other supposition will necessarily give a larger sum of squares of errors. The method of least squares is, then, the most general mode of finding the true magnitude from a number of divergent measurements; but when these measurements involve one magnitude only, the simplest mode of applying the method is to take the arithmetical mean.

CHAPTER VII.

EXPLANATION OF THE GIVEN.

159. Nature of Explanation.

Explanation is possible only if the given is necessary.

We saw in the first chapter of the present Book (*see* § 142) that the world only becomes intelligible when it is conceived as a systematic unity, the elements of which are throughout in necessary relations to each other. The attainment of knowledge is nothing but the more thorough and complete determination of these relations. Hence, every element of reality is understood just to the extent to which its necessary relations to other elements are grasped. No doubt, reality as presented to us is infinitely complex, and consists of various phenomena which resemble each other generally, yet differ indefinitely in detail. But the very postulate of knowledge compels us to think every variation and every detail, even the smallest, as so determined by conditions that, under the circumstances, it could not possibly be other than it is. That the given is necessary is an assumption without which it would be helpless to attempt to explain it, for all explanation resolves itself into ascertaining the exact conditions by which the given is determined. When the conditions of every detail of a phenomenon are so fully and exactly known that not only a phenomenon of this general character, but just this very phenomenon, with exactly these details, and each in exactly this amount, must follow from those conditions, and from those only, then that phenomenon is fully explained. Doubtless, in the vast majority of cases such thoroughness of explanation is not attained, and is not even sought. It is some particular aspect, or aspects, of the whole complex par-

It consists in the ascertainment of necessary conditions.

ticular fact that it is desired to explain, and much of the rest of the detail is passed over as having no bearing on the purpose in hand. But the ideal of explanation is the same: it is thorough in so far as the given can be shown to be the necessary consequence of certain definite and necessary conditions. It follows that all explanation is stated in those reciprocal hypothetical propositions which it is the business of induction to establish. The whole investigation in the present Book has, therefore, been an enquiry into explanation. And it may be gathered from it that explanation may be more or less perfect, that it proceeds throughout by hypothesis, and that it consists essentially in detecting the universal in the particular. For every relation is universal; wherever the conditions are realized there the consequence is also realized. Thus, all explanation is a detection of the universal in the particular; is, that is to say, a process of generalization. As explanation advances, therefore, phenomena widely different in many ways are seen to be, in some of their aspects, expressions of the same relation; the interdependence of the elements of reality becomes more apparent, and not one phenomenon only but many are frequently covered by the same explanation. Every hypothesis is an attempt at explanation; every established theory is an explanation of greater or less scope [*cf.* § 142 (iii.)].

All induction is explanation, and both are generalization.

Explanation may be directed either towards particular phenomena or towards those empirical laws which express ascertained coincidences and sequences of phenomena, often of considerable generality, the causes of which are unknown. But in each case the explanation consists in the definite determination of conditions [*cf.* § 150 (ii.) (*b*)]. Often, indeed, the only attainable explanation of a particular phenomenon is to show that it is an instance of some empirical law; but such explanation is far from complete. On the other hand, the explanation of an empirical law frequently consists in the establishment of one of those far-reaching statements of necessary relation, such as the law of gravitation, the principle of the conservation of energy, or the atomic theory of matter, by which the systematic unity

Explanation may be either of particular phenomena or of empirical laws.

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of the world is constituted, and which may be regarded as ultimate laws of nature.

Explanations are suggested by analogy.

The explanation of particular phenomena or classes of phenomena which are unfamiliar is, as we saw in treating of the modes in which hypotheses originate (*cf.* ch. iii.), frequently suggested by analogy, and it is successful when it can be shown that both the new phenomenon and that already known are determined by conditions which are, at least in some essential points, identical. To any one particular person, of course, explanation can only be conveyed in terms which are already intelligible to him. It is this which Clifford has in view when he says: "The explanation "describes the unknown and unfamiliar as being made up of "the known and the familiar," and he adds "By known and "familiar I mean that which we know how to deal with, "either by action in the ordinary sense, or by active "thought" (*Lectures and Essays*, pp. 101-2). He gives as an instance the explanation of the law of pressure of gases by the atomic theory. "The explanation [consists] in sup-"posing that a gas is made up of a vast number of minute "particles always flying about and striking against one "another, and then showing that the rate of impact of such "a crowd of particles on the sides of the vessel containing "them would vary exactly as the pressure is found to vary. ". . . Observe now; it is a perfectly well-known and "familiar thing that a body should strike against an opposing "surface and bound off again; and it is a mere everyday "occurrence that what has only half as far to go should be "back in half the time; but that pressure should be strictly "proportional to density is a comparatively strange, un-'familiar phenomenon" (*ibid.*, p. 101). Again, he says: "It is an explanation of the moon's motion to say that she "is a falling body, only she is going so fast and is so far off "that she falls quite round to the other side of the earth, "instead of hitting it; and so goes on for ever. But it is "no explanation to say that a body falls because of gravita-"tion. That means that the motion of the body may be "resolved into a motion of every one of its particles towards

Explanation may be logical or merely popular.

"every one of the particles of the earth, with an acceleration "inversely as the square of the distance between them. But "this attraction of two particles must always, I think, be less "familiar than the original falling body, however early the "children of the future begin to read their Newton" (*ibid.*, pp. 102-3). These extracts illustrate clearly the distinction between what we may call objective or logical explanation on the one hand, and popular or individual subjective explanation on the other. The latter looks at the individual mind, and seeks to bring the new phenomenon into relation to something already there; the explanation will vary in character with the person to whom it is addressed, and in many cases, will be utterly inadequate from a scientific point of view; it may be merely a rough analogy or illustration, as is constantly the case with the only explanations which can be given to children. Logical explanation, on the other hand, does not regard the individual mind as such, though, of course, it is reached and expressed by some individual mind. It expresses the explanation in terms of the most advanced knowledge on the subject, and it is itself, when first grasped, an addition to knowledge. The explanation by the atomic theory of the law of pressure of gases, and that of the moon's motion by gravitation are explanations of the latter kind, whilst the illustration of the former by rebounding bodies, and the statement of the latter as a movement of continual falling are examples of the popular kind of explanation. It is with the former kind of explanation only that logic has to do, and it is not too much to say that induction is concerned with nothing else.

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160. Generalization.

(i.) Nature of Generalization.

Generalization is often said to be identical with Induction.

From what has been said it is apparent that a process of generalization is throughout closely connected with induction. The two have, indeed, often been regarded as synonymous terms. Thus Dr. Venn says: "It has always been

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"held that Induction was essentially a process of generalization from particulars, and nothing more than this" (*Emp. Log.*, 1st Ed., p. 343), whilst Mill declares that "Induction properly so called . . . may . . . be summarily defined as Generalization from experience" (III., iii., § 1). In examining, therefore, the different views which have been held as to the nature of inductive inference, we have *ipso facto* dealt with the various opinions as to the character of the inference involved in generalization (*cf.* Ch. II.). But now that what we regard as the true doctrine of induction has been developed with some fulness, it may be profitable to enquire into the connexion of the process with generalization, and to determine the real nature of the latter form of inference.

The popular doctrine is that generalization is an inference from 'many' to 'all,' based on resemblance and dependent for validity on number of instances.

The popular doctrine is that generalization or induction is an inference from 'many' to 'all.' This was the meaning attached to the terms by the scholastic logicians, who made generalization consist in the establishment of the concept of a class by the omission from the observed particulars of all attributes which distinguish them from each other. Similar in essence is the doctrine that generalization is the inferring that what is true of observed particulars is true of other particulars which resemble them. This is the view advanced by Mill in the Second Book of his *Logic*. "A general truth," he says, "is but an aggregate of particular truths," and further "Generalization . . . is . . . a process of inference. "From instances which we have observed, we feel warranted "in concluding that what we found true in those instances, "holds in all similar ones, past, present, and future, however "numerous they may be" (II., iii., § 3). The force of the inference, then, depends on the number of the observed instances. But, as we saw in the last chapter [*see* § 157 (ii.) (*d*)], such an inference can never be more than probable. A consideration of what is usual can never give more than a strong expectation of continuance; it can never attain what science is always seeking, a knowledge of absolutely universal relations. This is insisted on by Jevons. "There "is," he says, "no distinction but that of degree between what

Such an inference can never be more than probable.

"is known as reasoning by *generalization*, and reasoning by "*analogy*. In both cases from certain observed resemblances "we infer, with more or less probability, the existence of "other resemblances. In generalization the resemblances "have great extension and usually little intension, whereas "in analogy we rely upon the great intension, the extension "being of small amount" (*Pr. of Sci.*, p. 596). By 'generalization' Jevons here apparently means little more than induction by simple enumeration. But later on he correctly tells us: "The term generalization, as commonly used, in- "cludes two processes which are of a different character, but "are often closely associated together. In the first place, we "generalize when we recognize even in two objects a common "nature. We cannot detect the slightest similarity without "opening the way to inference from one case to the other. ". . . A second process, to which the name of generalization "is often given, consists in passing from a fact or partial "law to a multitude of unexamined cases, which we believe "to be subject to the same conditions" (*ibid.*, pp. 597-8). But neither of these uses corresponds to that in the former passage unless we there interpret 'resemblance' to mean 'identity in essential conditions.'

Generalization is the detection of the essential conditions of phenomena.

This leads us to the true view that generalization consists in passing from observed phenomena to their essential and invariable conditions; in the detection, as Jevons puts it, of a true "common nature." When this is done, and when it is shown that any new phenomena are determined by just those conditions, all is done. There is no inference from observed instances to all; for once it is established that the essential conditions in the several cases are identical, no place remains for inference; we have a simple example of the law of identity. As Green tersely puts it: "From mere resem- "blance of attributes there is no valid inference at all; and "where for such resemblance has been substituted an identity "between the conditions . . . the inference is over" (*Phil. Works*, vol. ii., p. 279). Nor is the inference made from number of instances as such. As soon as the essential conditions of any phenomenon are ascertained, the generaliza-

The inference is not based on number of instances,

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tion that these conditions universally determine this result is made, and is expressed in the hypothetical judgment, *If S is M it is P*, and no amount of subsequent confirmatory experience strengthens it. And this complete knowledge may, in favourable cases, be attained by the careful analysis of a single instance. The validity of the generalization rests, not on the number of the instances examined, but upon the fundamental postulate of knowledge that every element of reality is definitely determined, and that always in the same way, so that the relation between a phenomenon and its conditions cannot vary. Without this assumption, as we have seen, all knowledge would become impossible. This, indeed, is the view of generalization that underlies Mill's treatment of Induction in the Third Book of his *Logic*, and is exemplified in his experimental methods, which are intended as means of ascertaining the essential conditions of phenomena, by eliminating all unessential circumstances (*cf.* § 155).

but on the assumption of uniformity of determination in reality,

The essence of generalization, then, is the establishment of universal hypothetical propositions stating the determinating conditions of any given phenomenon; and such propositions can be extended to new cases only as those cases are shown to be determined by just those conditions. But this is to say that generalization does not proceed from the known to the unknown, as no instances can be brought under the universal judgment till they are known. The universal hypothetical judgments of generalization, however, are purely abstract, and must be applied to reality by showing that there are cases which exemplify them. There must, that is to say, be material as well as formal generalization. It is not enough to know that *If S is M it is P;* for the advance of real knowledge we want to know *when* S is M.

and is expressed in a hypothetical proposition.

Generalization to new instances is only possible when their conditions are known.

In both these steps of generalization there is room for error. In the formal generalization error may exist, in that *S is M* may be an imperfect statement of the essential conditions of *P*, or may contain other elements as well as those essential conditions. The generalization is absolutely justified only when *S is M* does not *contain*, but *is*, the ground

Both the formal and the material processes are liable to error.

of *P*. The whole of the last two chapters have been devoted to an examination of the means of securing this. In the material aspect of the generalization there is also room for error. The point here is whether the essential conditions in this new case *are* exactly those expressed by *S is M*. The determination of this depends on insight in the particular branch of science concerned; it is not a part of logical theory, but of experimental practice.

The material step of generalization is foremost in the popular idea.

It is the material step of generalization which is foremost in the popular idea of inference through resemblance from many to all. No doubt, phenomena which are determined by the same essential conditions are frequently very similar in their sensible qualities, and in this resemblance we find practical guidance. All classification more or less assumes it, and we do not feel called on to analyse the conditions of each new object brought before us. As Dr. Bosanquet says: "No analysis of water would help us, however true the conditions under which it was made, if something which we "could not distinguish from water except by renewed "chemical analysis were liable to arise out of water by a "concealed process of causation, and were endowed with the "properties of sulphuric acid" (*Logic*, vol. ii., pp. 173-4). But even strong outward resemblance sometimes misleads us, as the issuer of false coin and forged bank notes is well aware. In scientific—*i.e.* exact—thought, we can never trust to it. A reliance on resemblance, for example, would class the sea-anemones in the vegetable rather than in the animal kingdom, and indeed for many years biology contained several instances of such false generalization. On the other hand, the outward resemblance may be of the slightest, and yet the essential conditions be identical. Jevons gives a striking example: "Between the brilliant explosive discharge of a thunder-"cloud and the gentle continuous current produced by two "pieces of metal and some dilute acid, there is no apparent "analogy whatever. It was therefore a work of great im-"portance when Faraday demonstrated the identity of the "forces in action" (*Prin. of Sci.*, p. 612).

Science must, therefore, always aim at reaching a full

Empirical generalizations precede generalizations by determination of conditions.

knowledge of essential conditions. But this aim cannot be attained all at once. As we have seen, the general process is from enumerative induction, through analogy, to complete scientific induction; and at all stages we have hypotheses, *i.e.*, tentative generalizations. Enumerative induction starts with resemblance, and suggests an empirical generalization that *Every S may be P*. But that the resemblance is felt to be due to an underlying identity is shown by the attempt made in analogy to reach this identity, and to find a possible ground for the empirical generalization. The further inductive process is but the testing, moulding, and limiting of the suggested ground until the ideal is reached of a statement of conditions so exact that for every variation in the phenomenon an explanation can be found in a modification of the conditions. But even in science the attainment of such absolutely universal judgments is only possible after long and careful investigation, and in many branches of knowledge scarcely any such are yet attained. Whether a branch of science has succeeded or not in so stating its ultimate principles, forms the distinction between what Mill calls 'Deductive and Experimental Sciences' (II., iv., § 5), a distinction which would be better expressed by speaking of 'branches of science in the stage of hypothetical enquiry or in that of the application of established theories.' And the one precedes the other. Geology and medicine are largely in the former stage, astronomy and physics chiefly in the latter, while chemistry occupies at present an intermediate position. Whilst a branch of science is in the stage of hypothetical enquiry its generalizations are more or less empirical. In other words, an exact knowledge of what has to be explained must precede an attempt to explain it. Empirical generalizations, then, naturally precede generalizations of strict determination, or, as Whewell puts it: "Inductive "truths are of two kinds, *Laws of Phenomena* and *Theories* "*of Causes.* It is necessary to begin in every science with "the Laws of Phenomena; but it is impossible that we "should be satisfied to stop short of a Theory of Causes" (*Nov. Org. Ren.*, p. 118). As was pointed out in discussing

hypotheses, the distinction between these is one of degree, not of kind [*see* § 150 (ii.) (*b*)], but it is not an unimportant one nevertheless. We must, therefore, consider each of these two classes of generalizations, remembering that the latter alone fulfil the logical ideal.

(ii.) Empirical Generalizations.

An empirical generalization is descriptive, not explanatory,

An empirical generalization is one which is simply descriptive and not explanatory. It states an observed uniformity either as to the relations between two phenomena or as to the mode of action of a certain class of objects, but the grounds of this uniformity are wholly or partially unknown. Kepler's laws of planetary motion before they were explained by the theory of gravitation were examples of the former kind of empirical generalization; that the tides recur at regular intervals was, before its explanation by the same great theory, an instance of the latter kind. Contemporary knowledge furnishes numerous examples of similar observed, but not explained, uniformities. Frequently they exist as empirical generalizations a long time before they are explained, but explanation is always regarded as ultimately attainable. And this belief is based upon a realization of the necessary unity of nature. For where there is observed uniformity of occurrence there is always a presumption that the conditions determining the phenomenon are constant. This, as we saw in our consideration of the doctrine of Probability [*see* § 157 (ii.) (*d*)], is the basis of all expectation of the recurrence of an event whose determining conditions are unknown, and this expectation rationally increases with the extension of uncontradicted experience of the event. In other words, the oftener an empirical generalization is exemplified, the greater is the probability that there is a necessary and universal relation underlying it. But whilst the generalization remains empirical, it must never be assumed to be universally and necessarily true. Its statement can never be more than categorical; we can at most say *Every S is P*, not *If S is M it is P*. Hence, the logical rule that an empirical generaliza-

but explanation is always regarded as attainable

Empirical generalizations can only be extended to cases similar in time, place and circumstances.

tion can only be safely extended to adjacent cases, *i.e.*, to cases which do not apparently vary much from those observed, and which do not exceed those limits of time and space within which observation has been made. As we saw in the estimation of the probability of an event's recurrence, the wider the scope over which we try to extend an empirical generalization the less is the assurance that it will be found to hold true, for the less ground have we for assuming that the essential determining conditions will remain unchanged.

Statistical uniformities are only empirical generalizations,

An interesting class of empirical generalizations is composed of what are often called 'Statistical Laws' but which are better termed *Statements of Statistical Uniformity*. It is well known that the number of persons who commit certain crimes, who are born, or who die, in the course of a year bears a remarkably uniform proportion to the total number of the inhabitants of any given country; there is, as we say, a pretty constant *average* preserved in many of the phenomena of social life. For example, something over seventy people out of every million commit suicide every year in England and Wales, whilst in Saxony the proportion is about five times as great as this, and in Ireland only about one-third as large. And these numbers are found to remain very uniform from year to year. Moreover, the averages are found to vary with great regularity according to the months of the year, being highest in June, falling regularly to December, and then gradually rising again; and this occurs year after year. Further, the proportion who commit suicide at different ages remains fairly constant. This all points to the fact that social and material conditions remain comparatively unchanged for mankind in general. But it is an error to assume that such a statistical uniformity proves anything beyond its own existence. If it occurs again we are justified in concluding that the same causes have been at work in society as a whole. But with regard to the future we can only judge that the same numbers will be found to hold, if the same general conditions remain. To speak of such a uniformity as a 'law' which must of

necessity be fulfilled is to misuse language, for all a 'statistical law' can say is, *If all* conditions remain unchanged for the next period of time, then will all their consequents be realized. But this is mere tautology, for it simply says, If the past be exactly repeated it will be exactly repeated. The very question is whether all the conditions—known and unknown—will remain unchanged. But from the constancy of such averages an inference as to their necessity is sometimes drawn. Thus Buckle says: "In a given state of "society a certain number of persons (about 250 each year) "must put an end to their own life. This is the general "law, and the special question as to who shall commit the "crime depends of course upon special laws: which how-"ever, in their total action, must obey the large social law "to which they are all subordinate. And the power of the "larger law is so irresistible, that neither the love of life, "nor the fear of another world can avail anything towards even "checking its operation" (*Hist. of Civilization*, vol. i., p. 25). But uniformity in averages combined with irregularity of details is exactly what the theory of probability would lead us to expect, even if no causation was operative at all, and it involves no necessity for its own continuance. No doubt every element of reality is strictly determined in all its details by its conditions—given exactly those conditions that result is necessary. But this necessity is concealed by the average, which neglects all the particular characteristics of the individual instances. Necessity is only found when conditions are exactly determined, and *ex hypothesi* that is just what is not done in the case of an average. As Sigwart says: "Such uniformities of numbers and averages are "primarily mere descriptions of facts, which need explana-"tion as much as the uniformity of the alternation between "day and night; and the explanation can be found only "where the actual conditions, the efficient causes, are forth-"coming. But these are the concrete conditions of the "particular instances counted, they are not directly causes "of the numbers; it is only the nature of the concrete "causes which can show it to be necessary for the effects to

and involve no necessity for their continuance.

"appear in certain numbers and numerical relations" (*Logic*, Eng. trans., vol. ii., p. 490).

Law is ambiguous—two main senses are: (1) Judicial. (2) Scientific.

The consideration of this leads us to notice briefly the confusion which may be caused by the ambiguity of the word *law*. In the passage quoted from Buckle the 'law' is spoken of as having a constraining force. This is its judicial use, and one which is quite inapplicable to mere observed uniformities. This use of the word is thus defined by Austin in his *Lectures on Jurisprudence*: "A law, in the "most general and comprehensive acceptation in which the "term, in its literal meaning, is employed, may be said to "be a rule laid down for the guidance of an intelligent being "by an intelligent being having power over him." But in the scientific sense, a law is a statement of a necessary connexion; it is not something imposed upon reality from without, but is the outcome of the nature of reality itself. The perception of natural laws is due, no doubt, to the synthetic activity of mind, but it is possible only because the connexion actually exists in reality. It is because the world is a systematic unity that we are compelled to think it as such. A law, then, in the strict scientific sense is a fully established statement of universal and necessary connexion. Such a connexion is, however, always abstract; no law ever expresses more than one aspect of a phenomenon; thus the law is the means by which we mentally interpret the fact, but is never simply and exactly exemplified in the fact.

Empirical generalizations are not laws.

But more loosely, the term is frequently used to denote mere empirical generalizations such as we have been considering. These, however, express no necessity and, consequently, are not laws in the strict sense. It is better, therefore, not to apply the term to them at all, or at least, to qualify it if it is so applied by the adjective 'empirical'; though indeed 'empirical law' is, correctly speaking, almost a contradiction in terms.

(iii.) Necessary Truth.

We have seen that a general judgment is not a summary of particular judgments, for it is not arrived at by summariz-

ing the records of events which have been often observed to occur. Such a process, as we have more than once urged, can yield nothing more than a greater or less degree of expectation. General truths are, then, not derived from experience, in the sense in which the empiricist uses the word; that is, they are not obtained from simple observation of the actual connexion of two phenomena. On the contrary, they are the result of such an analysis of reality as enables us to state the relation which holds between a phenomenon and its conditions. And when this relation is ascertained, the principle of the unity of the world—without which all knowledge is impossible—compels us to think it as universally and necessarily true. Of course, if our analysis is inaccurate, if what we assume to be the conditions of the phenomenon are not really its conditions, then our general propositions are not true. Or again, if our analysis is inadequate; if what we take for the essential conditions include other elements, or if we omit some essential elements of the conditions, then our judgment has not universal validity; it is, we may say, nearly true. There is, then, room for mistake in the application of our hypothetical judgments to reality. Every such judgment is subjectively necessary as an act of thought; it expresses a necessary relation between its antecedent and its consequent, and whoever accepts the judgment, *ipso facto* accepts this relation as universal and necessary. But the judgment may fail to express accurately the nature of reality, either concerning the particular relation with which it deals, or respecting the connexion of that relation with systematic reality as a whole. However, in so far as any general judgment is true, it is necessarily true, for necessity consists only in the connexion of a consequent with its ground. Uncertainty, when it exists, pertains to the truth of the judgment in the form in which it is expressed, *i.e.* it attaches to the accuracy and adequacy of the analysis of conditions upon which it is founded. When this accuracy and adequacy are undoubted there is no uncertainty.

General judgments result from analysis of phenomena.

When the conditions of a phenomenon are accurately stated the judgment is necessarily true.

Doubt can only affect the statement of the conditions.

Here we find the reason why the axioms and other truths of mathematics are often held to be of a superior rank to

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Mathematical truths are certain because all the conditions are known.

those of physical science; so that the former are spoken of as 'necessary,' the latter as 'contingent' truths. Now mathematical truths are not necessary in the psychological sense that everybody must think them; as a matter of fact the majority of the inhabitants of the world never do really and explicitly think such truths. They are necessary in the sense that those who think them at all can think them only in one way. For in mathematics we deal entirely with abstract relations; we, therefore, constitute the conditions by our own mental act, and as so constituted they are not liable to be modified by other conditions. In geometry, for example, we deal only with abstract ideas of limits—with surfaces, and lines and points. We construct our figures by a mental synthesis of just those elements and nothing more, and from such construction we derive propositions which, as they express relations subject to no conditions but those which we have ourselves imposed, are applicable to every other figure in which just those conditions, and those only, are fulfilled, and are, therefore, universal. Indeed, in mathematical construction the ideal aimed at in experiment—to have no unknown conditions present—is realized.

Mill is wrong in holding that the certainty of mathematical truths is fictitious,

It is impossible, therefore, to agree with Mill when he says that the "character of necessity, ascribed to the truths "of mathematics and even (with some reservations . . .) "the peculiar certainty attributed to them, is an illusion; in "order to sustain which, it is necessary to suppose that "those truths relate to, and express the properties of purely "imaginary objects" (*Logic*, II., v., § 1). In support of this position, and in accordance with his general empiricist doctrine, he advances the theory that the definitions of geometry are "some of our first and most obvious generaliza-"tions concerning natural objects." This we cannot grant. The concepts and axioms on which mathematics rests are not generalizations from sensuous experience. Points and lines are not sensations or combinations of sensations, but are the conceived limits of surfaces, in other words relations which exist for thought but not for sense perception. After

that the definitions of geometry are generalized from natural objects,

laying down that "there exist no points without magnitude; "no lines without breadth, nor perfectly straight; no circles "with all their radii exactly equal, nor squares with all their "angles perfectly right," Mill goes on to say "The points, "lines, circles, and squares which anyone has in his mind, "are (I apprehend) simply copies of the points, lines, circles, "and squares which he has known in his experience. Our "idea of a point, I apprehend to be simply our idea of the "*minimum visibile*, the smallest portion of surface which we "can see. A line as defined by geometers is wholly incon- "ceivable. We can reason about a line as if it had no "breadth; because we have . . . the power, when a percep- "tion is present to our senses or a conception to our in- "tellects, of *attending* to a part only of that perception or "conception, instead of the whole. But we cannot *con-* "*ceive* a line without breadth; we can form no mental "picture of such a line" (*Logic*, II., v., § 1). Hence, each geometrical definition "is only nearly true; so nearly "that no error of any importance in practice will be in- "curred by feigning it to be exactly true" (*ibid.*). Similarly he tells us the axioms of geometry "are experimental truths; "generalizations from observation. The proposition, Two "straight lines cannot enclose a space . . . is an induction "from the evidence of our senses" (*ibid.*, § 4). But, if 'experience' gives us no lines which are perfectly straight, whence comes the idea of a straight line? Mill says that the lines of experience approximate to straightness, and therefore, apparently, that the idea of straightness is obtained by abstracting all elements of crookedness from the imperfect lines presented to our senses. But this is to beg the question. For crookedness is known only as deviation from straightness; unless, therefore, we approach crooked lines with the idea of straightness already in our minds, it is evident we cannot know them as more or less crooked, for we have no standard by which to test them. And we cannot 'abstract' straightness from the lines given in sense perception, for *ex hypothesi* it is not there to be abstracted. Similarly we must have a concept of a perfect circle before we can say

and are only nearly true,

and that geometrical axioms are generalizations from observation.

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Mill confuses conception with imagination

that any 'real' circle is, or is not, imperfect. Mill's whole argument, indeed, is vitiated by the confusion between a true *concept* and a mental *image*. Throughout his discussion of the definitions of geometry, he is really speaking of images, or sensible representations of objects possessing certain attributes, instead of concepts, *i.e.* intelligible syntheses of those attributes. Thus we get the sufficiently absurd paradox that "a line as *defined* . . . is *inconceivable*." How the definition, which states the content of the concept (*cf.* § 49[1]), is possible, though the existence of the concept itself is impossible, Mill does not explain. That we cannot form mental images of perfect geometrical figures is true; that we cannot conceive—*i.e. think*—them is false. As G. H. Lewes says: "That which is unpicturable may be conceivable, "and the abstraction which is impossible to perception and "imagination is easy to conception. It is thus that sensibles "are raised to intelligibles, and the constructions of science— "conceptions—take the place of perceptions" (*Problems of Life and Mind*, vol. i., p. 420). Nor are these conceptions 'imaginary' in the sense of 'unreal' in which Mill uses the word. The truly 'real' figures of geometry are the accurate ones of conception, not the more or less imperfect ones of perception. And of these real figures the theorems of geometry are strictly and exactly true, whilst they are true of the material figures of sense experience just in the proportion in which those figures approximate to ideal perfection. The necessary character of mathematical truths, is therefore not due, as Mill says it is, to inseparable connexion in experience. This we have seen can only give expectation; it can never prove the truth of a universal proposition in any subject. As Dr. Bosanquet says: "It is of no use to say, "'I have seen it so often that I cannot help believing it "true.' One might almost as well say, 'I have said it so "often that I cannot help believing it true.' The question "is not how often you have seen it, but what you now know "that you saw, and under what precise conditions" (*Logic*, vol. ii., p. 223). It is, as was said above, because we can in mathematics state the precise conditions of any relation

The real figures of geometry are ideal constructions of thought.

The necessity of mathematical theorems is due to exact knowledge of conditions,

[1] First Edition, § 56.

that the theorems of that science are exactly and necessarily true.

But in physical science, the essential conditions are given us in complex reality; we have to find them and not merely to construct them; hence, there is, in many cases, room for some uncertainty as to whether they have been correctly ascertained. But, it is as true of a general judgment concerning natural phenomena as it is of a general judgment in mathematics, that if it is once true, it is always true, and that so far as it is true it is necessary in that system of reality of which alone we can form a conception. We will sum up this part of our discussion in the words of Green: "The distinction, then, of the 'necessity' of mathematical "truths from the 'contingency' of truths about nature, if it "is to hold at all, is not to be understood as if it were only "in mathematics, and not in natural Science, that what is "once true must be always true, or as if natural laws were "liable to change, mathematical laws not. The true distinc-"tion is between what is fully true and what is partially "true. What is fully true once is fully true always, of a "natural phenomenon no less than of a geometrical figure; "but any proposition about a natural phenomenon is true "of it only under conditions of which we do not know all, "while a proposition about a geometrical figure, if true at "all, is true of it under conditions which we completely "know" (*Phil. Works*, vol. ii., pp. 249-50).

but this complete knowledge is frequently impossible in physical science.

Still, all general propositions about nature are necessary in so far as they are true.

Now to say that a judgment is held as universally, *i.e.*, as necessarily, true is the same thing as to say that its contradictory is inconceivable (*cf.* § 18[1]). If one has clearly conceived the proposition and the grounds on which it rests, and has accepted it as true, it is impossible for him at the same time to think its contradictory as true. But, before the general proposition was conceived, it was, of course, possible to accept its contradictory. The fact, therefore, that propositions, whose contradictory cannot now be conceived by persons who are well-informed in the branch of knowledge to which they refer, have not been always held to be true, or are not now held to be true by everybody, is not a

To accept a judgment as true is to reject its contradictory as inconceivable.

The acceptance of a truth as necessary need not be universal.

[1] First Edition, § 23.

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valid argument against the position that the contradictory of a proposition received as true is inconceivable. We know that many judgments are now received as true by savages which are incredible to civilized men; and on the other hand, a scientist holds many judgments to be necessarily true which to a less well-informed mind are doubtful or meaningless. The variation is simply due to differences in knowledge. But to argue as Mill does [*Logic*, II., v., § 6; vii.] that a judgment cannot be said to be inconceivable because it has at one time been held to be true is inadmissible except on the totally unwarranted assumption that every true proposition must have been known by all men at all times. The true doctrine is simply that "Given any proposition conceived as "wholly (unconditionally) true, you cannot conceive its con-"tradictory to be true consistently with that idea of the "unity of the world without which no proposition could be "conceived to be really either true or untrue" (Green, *op. cit.*, p. 266).

Inconceivability of its contradictory is not an independent test of the truth of a proposition.

But, as holding a proposition to be necessarily true and holding its contradictory to be inconceivable are one and the same thing, it follows that inconceivability of the opposite is not, as Mr. Spencer teaches, an independent test of the truth of any proposition. Even an individual who accepts a proposition does not hold it to be true because he cannot conceive its contradictory. The fact that he cannot do so shows that every judgment is for him who makes it necessarily true. But this necessity is purely psychological. It is not, as Dr. Bosanquet says, "the logical "reason for making the judgment, only a psychological "description of the effect which that reason produces" (*Knowledge and Reality*, p. 332). The logical ground or reason why any individual holds a judgment to be true can only be found in the relation of that judgment to the totality of knowledge of the individual.

Logical necessity is relative to the whole system of knowledge,

If we wish to pass from this individual necessity we can only do so on the same lines. We must take as the ground of the truth of a judgment its relations to the whole system of knowledge available. Thus we may say that a general proposition is objectively true and logically necessary when it expresses the only possible way

of explaining the facts consistently with the conceived unity of the world. As this necessity is grounded in the whole system of knowledge, it follows that as that system becomes more complete, the probability that any judgment received as true by experts in any branch of knowledge will ultimately be proved to be false decreases. But it is also evident that such necessity cannot be absolute and ultimate until knowledge is complete. For, as all the elements of reality are interrelated through and through, so that the whole universe is a systematic and organic unity, it follows that perfect validity of thought would involve perfect knowledge of the universe. As Dr. Bosanquet says: "Ultimately, "we may imagine, nothing can be rightly known without "knowing all else rightly" (*Logic*, vol. i., p. 393). But as we are yet far from this state of omniscience, we must acknowledge that our judgments about reality are tainted with a greater or less degree of uncertainty, and that the truth of our universal judgments is, to some extent, provisional and dependent upon the advance of knowledge. But so far as a judgment is true our fundamental postulate of knowledge compels us to think it as necessary; doubt does not attach to the universal determination of a phenomenon by its conditions, but to the accuracy and adequacy with which our analysis has ascertained those conditions.

and can only be absolute when that system is complete.

161. Systematization.

Our discussion has led us to see that the ultimate aim of scientific thought is to attain a consistent and adequate concept of the universe as a systematic whole, and that we approximate more and more closely to this as we find more and more fully that the phenomena of experience are reducible to manifestations of law. But every law is applicable to a multiplicity of facts, and expresses one of its aspects, and hence scientific thought is, as Mach puts it, "economical" and makes it possible to mentally grasp in one compendious view a mass of detail which would otherwise be utterly unmanageable. The grasp of laws alone can make experience intelligible to us; as we said in the

Scientific thought aims at conceiving the universe as a systematic whole,

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beginning of this book, a fact without the idea by which it is thought, is nothing [*see* § 141 (ii.)]. Moreover, scientific thought aims at reaching laws of the widest possible application. As Mach says: "Those ideas that hold good throughout the widest domains of research and that supplement "the greatest amount of experience, are the *most scientific*" (*Princ. of Mechanics*, p. 490). And this bringing more and more of the realm of experience under one and the same law is, in a very true sense, a simplification of our concept of the universe, and the only sense, as was previously pointed out, in which simplicity is a test of the truth of a theory (*cf.* § 152).

and tries to reach laws of wide application.

The Principle of Continuity is found to hold generally in nature.

One of the widest-reaching and most fertile of the ideas of which physical science avails itself is the Principle of Continuity, expressed by the old dictum *Natura non agit per saltum;* which assumes that no abrupt transitions are ever found in nature. By this principle, *e.g.*, though experience can never give us an exact instance of the operation of the law of inertia, yet we may approximate nearer and nearer to one, as the interfering conditions of friction, etc., are more and more reduced, and so we may justly infer what experience can never show us, that if all interfering conditions were removed, the law would be exactly fulfilled in phenomena. And the same holds with many other laws. The discovery of the existence of 'critical pressures' and 'critical temperatures' of liquids has revealed continuity even where it seemed to fail, viz., in the transition from the liquid to the gaseous state, which under ordinary circumstances appears an abrupt breach of continuity. But experiment has shown that at certain temperatures and under certain pressure, differing for various substances, the substance experimented on exists in a state which is at the same time indistinguishable from that of liquid and gas. For example, the critical temperature of hydrogen is $-220°$ and the critical pressure is 20 atmospheres [*cf.* § 154 (ii.) (*f*)]. Similarly, accurate observation has shown that "ice does not turn into water all at "once, but through a small fraction of a degree the change "is gradual" (Jevons, *Pr. of Sc.*, p. 619). It is evident that

this principle largely conduces to the economy of thought and is fertile in suggesting explanations of phenomena.

The mechanical theory of the universe is simple, but inadequate even in inorganic nature;

In the attempt to think the whole universe under one concept, the simplest and most apparently obvious is that of a thorough-going mechanism. Thus, assuming the atomic theory, and the theory of the conservation of energy, all phenomena, it is said, may be conceived as due to mechanical relations of the ultimate atoms. But, even in the inorganic world it is doubtful whether this conception is adequate. As Mach puts it: "Purely mechanical pheno-"mena do not exist. . . . [They] are abstractions made, "either intentionally or from necessity, for facilitating our "comprehension of things. The same thing is true of the "other classes of physical phenomena. Every event belongs, "in a strict sense, to all the departments of physics. . . . "The view that makes mechanics the basis of the remaining "branches of physics, and explains all physical phenomena "by mechanical ideas, is in our judgment a prejudice. . . . "We have no means of knowing, as yet, which of the "physical phenomena go *deepest*, whether the mechanical "phenomena are perhaps not the most superficial of all, or "whether all do not go *equally deep*. . . . The mechanical "theory of nature is, undoubtedly, in an historical view, "both intelligible and pardonable; and it may also, for a "time, have been of much value. But, upon the whole, it is "an artificial conception" (*op. cit.*, pp. 495-6). Of course, the theory of the conservation of energy does not commit one who accepts it to a merely mechanical view of nature, as it simply assumes that there is "an invariable quantitative "*connexion* between mechanical and other kinds of pheno-"mena" (*ibid.*, p. 499). Moreover, "all physical knowledge "can only mentally represent and anticipate compounds of "those elements we call sensations. It is concerned with "the connexion of these elements. . . . Processes, thus, that "in appearance are purely mechanical, are, in addition to "their evident mechanical features, always physiological, "and, consequently, also electrical, chemical and so forth. "The science of mechanics does not comprise the founda-

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"tions, no, nor even a part of the world, but only an *aspect* "of it" (*ibid.*, p. 507).

in organic nature it must be supplemented by the principle of development,

When we turn our attention to what is known as organic nature the inadequacy of the mechanical theory becomes more evident. Here we need, at the least, to supplement any such theory by the principle of development. But this principle involves some reference to an end, and is, therefore, to some extent at any rate, teleological. For in tracing the development of an organism, the lower and earlier forms are necessarily regarded as conditions, together with the environment, of those higher and final forms, whose adaptation to their environment is most perfect. Hence, the principle of development compels us to seek the true nature of organisms, not in the past states from which they have been evolved but, in the future states towards which they gradually and more or less perfectly approximate.

and finally by the conception of rational purpose.

But this constant though gradual adaptation to an end is only quasi-teleological, as the end is not conceived by the developing organism. The concept of the universe as the expression of rational thought, as, consequently, comprehensible by intelligent beings, and, as a result of such comprehension, adaptable to their needs and purposes, is, however, fully teleological. This conception embraces and transcends the others, and seems to us to be the only one in which the mind of man can find rest and satisfaction.

BOOK VI.

METHOD.

CHAPTER I.

GENERAL NATURE OF METHOD.

162. Scope of Method.

BOOK VI. Ch. I.

Method is the correct arrangement of thoughts.

In the last three Books we have considered under what conditions inferences are valid. But correctness of inference is not all that is necessary either for the attainment of truth or for its exposition; it is also essential that those inferences should be properly combined. This is the subject-matter of the doctrine of Method, which, in the words of the late Professor Croom Robertson, considers "how reasonings, when employed continuously upon any matter whatever, should be set forth to produce their combined effect on the mind" (*Ency. Brit.*, 9th ed., vol. i., p. 797).

Method may, therefore, be defined as *the correct arrangement of thoughts either for the discovery or for the exposition of truth.* This is, obviously, a subject of the utmost importance, but it is at the same time one which, from its character, is incapable of being reduced to strict rule. There is, therefore, much less of definite doctrine to be given in this than in the other divisions of logic, and our task will be limited to the general considerations and principles on which good arrangement depends.

BOOK VI. Ch. I.

163. Kinds of Method.

The character of the method depends upon whether we begin with the particular or with the general.

The character of the method to be adopted is partly determined by the subject-matter dealt with, and partly by the object we have in view. This will be manifest from our discussion of induction and its relation to deduction. For in some cases we can begin with general judgments and principles, and our object is to follow these out into their application in particular instances; and in other cases, we must approach the problem from the side of particular instances, and our object is to discover what general laws those particulars exemplify. There is, thus, a broad division of method, corresponding to the distinction between inductive and deductive inference [*cf.* § 146 (iii.)]. When the object is to ascertain general principles, and the only procedure open to us is the examination of particular instances, the method followed is one of *Analysis*. For here we start with the complex of experience, and by examination and thought try to analyse or resolve it into the simple and universal relations which are realized in it. On the other hand, when we can start from general principles, either intuitively grasped or rigorously demonstrated, we follow a method of *Synthesis;* for by the composition of the simple and universal relations we seek to construct the complex individual. But, as will be manifest from our previous discussion, these two methods are not opposed to each other. Rather they "differ "only as the road by which we ascend from a valley to a "mountain does from that by which we descend from the "mountain into the valley, which is no difference of road, "but only a difference in the going" (*Port Royal Logic*, p. 314).

In the former case the method is one of *Analysis;* in the latter case it is one of *Synthesis*.

The two methods are not opposed.

A science is *experimental* when its method is mainly analytic, and *deductive* when its method is chiefly synthetic,

Doubtless in some branches of knowledge the one method is more prominent than the other, and thus we have the distinction between deductive and experimental sciences, or, as it is sometimes expressed, between inductive and deductive sciences [*cf.* § 160 (i.)]. As Mill puts it: "A science is ex-"perimental, in proportion as every new case, which presents "any peculiar features, stands in need of a new set of obser-"vations and experiments—a fresh induction" (II., iv., § 5),

whilst it is deductive in proportion as every new case as it arises can be brought under general principles already established. Sciences which deal with concrete reality naturally start by being preponderatingly experimental, and as they approach towards perfection become more and more deductive. For as knowledge advances, wider and wider relations are established, connecting together results already arrived at, and bringing more and more unity into the whole department of knowledge. And as a science advances towards the deductive stage, in which the method is mainly synthetic, so it becomes more and more abstract, as it deals more and more with universal relations, and less and less directly with concrete individuals. The greatest step in transforming the general method of a science is when considerations of exact quantity are first made applicable (*cf.* § 156). The science is then brought into touch with mathematics, which is emphatically *the* deductive or synthetic science. Thus, for example, the science of astronomy has, since the time of Newton, been preponderatingly synthetic in its method, and the same may be said of physics. On the other hand, chemistry is still largely experimental, and geology and medicine still more so. But in no science is one method used exclusively. Geometry, for example, is generally synthetic in method, for it starts from certain definitions, axioms, and postulates, and deductively infers from them particular and complex spatial relations. But, when a theorem is proposed to a geometrician, and, examining its terms in detail, he works out the results of each, and so obtains a conclusion which establishes, or disproves, the original theorem, he is pursuing the method of analysis or induction by hypothesis.

but in no science is either method used exclusively.

One distinction commonly drawn between analysis and synthesis is that the former is the method of discovery, and the latter the method of exposition. And, interpreted carefully, this will hold. Analysis is certainly the method by which discovery at any rate starts, when it is concerned with concrete reality. But to say that all discovery is made by analysis would be to neglect the advances made in knowledge by the aid of mathematics, and to assert that as a

Broadly speaking, analysis is the method of discovery, and synthesis that of exposition.

Book VI. Ch. I.

science becomes more perfect—*i.e.* more synthetic in its method—its power of advancing knowledge decreases. On the other hand, the method of synthesis is the most appropriate for the exposition of truth to one who can grasp the first principles, or general propositions, from which that method starts. But it does not follow that it is the one most adapted for imparting new knowledge generally. To adopt it, indeed, would be, in many cases, to disregard the essential rule of method to start from the familiar (*see* § 164, Rule III.). Especially is this the case when we are dealing with children. To them the concrete particular is more familiar, and easier to be comprehended, than abstract general relations. Moreover, one great aim of the educator is to train in his pupils the power of making advances in knowledge without his aid. From both these considerations it follows that the right method to adopt in teaching children is, in its first stages, analytic. As analysis, however, in the growth of knowledge, gradually gives place to synthesis, and as nothing is thoroughly grasped till it can be reconstructed from its elements as well as resolved into them, so every step of analysis is, in good teaching, followed by one of synthesis. But when one's exposition is intended for well-prepared adults—as when one writes a text-book—the most appropriate method is, generally speaking, that of synthesis; as by that method the necessary relations of the parts of the subject to each other are most clearly shown. And, generally, the synthetic process is essential to success in the attempt to impart a clear and consistent view of any department of knowledge.

164. General Rules of Method.

Statement of five General Rules of Method.

Descartes, in his *Discourse on Method* (Part II.), gives the following four general rules of method, which have been substantially adopted by subsequent writers on the subject :—

Rule I. Never to accept anything as true which we do not clearly know to be so.

Rule II. To divide each of the difficulties under examination into as many parts as possible, and as may be necessary for its adequate solution.

Rule III. To conduct our thoughts in such order that, by commencing with objects the simplest and easiest to know, we may ascend by little and little, and, as it were, step by step, to the more complex ; assigning in thought a certain order even to those objects which in their own nature do not stand in a relation of antecedence and sequence.

Rule IV. In every case to make enumerations so complete, and reviews so general, that we may be assured nothing is omitted.

To these should be added another, viz. :

Rule V. To conceive clearly the end to be attained by the enquiry or argument.

These rules, from their very generality, are no doubt difficult of application, but as the writers of the *Port Royal Logic* observe, "it is always advantageous to have them in "the mind, and to observe them as much as possible when "we try to discover the truth by means of reason, and as "far as our mind is capable of knowing it" (Eng. Trans., p. 316).

Speaking generally, we may say that the essence of the rules may be summed up in the directions to make sure of our starting-point, to know the end we wish to attain, and to go from the starting-point to that end by orderly and consecutive steps, each of which is seen in its true relation to all the rest of the enquiry.

All good method starts with what is simplest to the person to whom the discourse is addressed,

The starting point must always be that which is simplest and most easy to be understood. But it is important to remember that simplicity is relative to previous knowledge, and that superior simplicity broadly resolves itself into superior familiarity. It is, thus, frequently the case that what is simplest in itself is by no means simplest to the

learner. From the point of view of comprehensive knowledge, an abstract law of wide generality is simpler than any of the particular facts in which it is exemplified; for each of those facts is complex, and contains much more than an expression of that law. In this sense the law of gravitation is simpler than the fall of an apple. But it by no means follows that it is simpler to one just entering on the study of nature. From the nature of the case, indeed, it cannot be so, for the general and abstract law is—as we saw in our consideration of induction—only arrived at as the result of a long and difficult analysis of complex reality. It, therefore, follows that this rule is adhered to only when the simplicity spoken of is determined by reference to the individual for whose sake the whole discourse is to be arranged. And it will be noticed that the decision of this point determines the general method to be adopted; for when the 'simple' from which we start consists of the complex particular facts of experience, the general method of the exposition is analytic, but when it is found in the enunciation of some easily grasped general truth—as, *e.g.*, in geometry—the general method is synthetic.

and the character of this determines the general nature of the method.

True sequence must be observed.

The importance of keeping to the true order of sequence, so that each step is made sure before the next advance is attempted, and so that each step follows naturally and easily from those that have preceded it, can scarcely be overestimated by those who would either learn or teach with success and thoroughness. For, as the Port Royalists say: "It is indubitable . . . that we learn with incomparably "greater facility, and retain much better, what has been "taught us in the true order; because the ideas which have "a natural connexion arrange themselves much better in "our memory, and suggest each other much more readily" (*op. cit.*, p. 344).

Examination of subject matter must be exhaustive and intelligent.

Closely connected with this matter of arrangement is the question of correct and exhaustive subdivision of the subject under investigation or discussion. This implies distinguishing the essential from the accidental and, for the purpose in hand, unimportant elements, and the correct

estimation of the relations of all the elements to each other. Experience here is apt to lead us astray. There is a natural tendency to regard as necessarily connected what in our experience we find uniformly conjoined. There may be, and often is, a reason for the conjunction, but that reason can only be found in a careful analysis of all the complex circumstances. Thus, to borrow an illustration from Mr. Clarke (*Logic*, p. 470), if it were proved by statistics that a larger number of people are drowned on Sunday than on any other day of the week, there would be a tendency amongst many people to regard the fact as a sign of punishment for the desecration of the Sabbath, thus overlooking the essential circumstance that many people who cannot swim find the opportunity on that day only of disporting themselves in boats they do not know how to manage. In the examination of social problems this matter of exhaustive analysis is all-important. An increase in the number of arrests for drunkenness, for example, does not necessarily demonstrate an increase of intemperance; it may equally well point to greater activity on the part of the police.

The end to be attained must be clearly grasped.

But the possibility both of orderly arrangement and of correct estimation of the relative importance of the elements involved depends upon a clear conception of the end the enquiry is meant to attain. Of course, when the aim of the investigation is to reach new knowledge, and not simply to convey knowledge new to another but familiar to the expounder, this conception of the end is not definite and precise knowledge; if it were there would be no room for the enquiry. But the nature and general character of the end to be attained must be clearly apprehended at the outset and kept in view throughout the discourse. Every step must then be a progressive filling out and determination of this end, till at length the problem is solved. No learner or enquirer can make true and regular progress by groping in the dark. Attention must be concentrated and kept to the point, ready to see the relevance or irrelevance of each new consideration, and to dwell on, or reject, it accordingly. And relevance to the end can only be estimated when

the nature of the end is known. This rule was given by the Port Royalists as applicable to analysis. It is however equally important when the method is synthetic, and must, therefore, be included in the general rules of method.

CHAPTER II.

ANALYSIS.

165. Application of the General Rules.

Analysis is the method of induction.

It will not be necessary to say much on the analytic method, as it is really the method of inductive inquiry which we examined in the last Book, and which we there saw is not confined to single steps of inductive inference, but often involves long and complex trains of reasonings, all advancing little by little towards the end of the enquiry, enunciated in the hypothesis which guides the investigation. We saw also that this examination of complex reality cannot be reduced to mechanical rules. As the Port Royalists acutely pointed out, analysis "consists more in judgment and sagacity of mind than in particular rules" (*op. cit.*, p. 315). But the general rules of method are all-important. Of course the very essence of analysis is that the 'simple' from which we start is found in particular facts of experience, and the end aimed at is more or less accurately and clearly conceived and expressed in the hypothesis which guides the investigation. The necessity and difficulty of an exhaustive and systematic analysis of the given facts, and of a correct estimation of their relative importance, were sufficiently dealt with in Chapter V. of the last Book. As the Port Royalists well remark: "It is in the attention we give to that which is "known in the question we wish to resolve, that analysis "mainly consists, the whole art being to derive, from this "examination, many truths which may conduct us to the "knowledge of what we seek" (*op. cit.*, p. 313).

The General Rules of method must be observed.

Book VI. Ch. II.

The analytic method of instruction is more direct than that of discovery.

In the process of actual discovery it happens frequently, as we have seen, that hypotheses are put forth which further enquiry shows to be either altogether wrong, or to need more or less extensive modification. But when the analytic method is adopted in teaching, none of these erratic movements of thought should find a place. The teacher, having traversed the way before, is enabled to guide the pupil to the desired goal by the straight and direct road without allowing him to be distracted into by-paths diverging from it. The analytic method of exposition, therefore, differs in one very important particular from the actual process of discovery. It is more direct, more definite, and, in consequence, much shorter. Did not the teacher thus shorten the road to his pupils, progress in knowledge would obviously be much slower than it is, as much time and energy would be expended in rediscovering all that has been already established. But, in the general essence of reaching general truths by an examination of concrete particulars, the two applications of the method are in agreement. The difference is that, in teaching, the particulars to be analysed are carefully selected, as specially adapted to exhibit the general law to be arrived at, by one who has already found that law in them; whilst in discovery the investigator has to work with material which has undergone no such preliminary process of selection, and the greater or less relevance of which is fully determined only in the course of the enquiry itself.

CHAPTER III.

SYNTHESIS.

166. Application of the General Rules.

Synthetic Method starts from generals and proceeds deductively.

In synthesis the 'simple' from which we start is found in general propositions accepted as true, and the progress of the argument is throughout deductive, and consists in the application of those general propositions to particular cases. All the general rules of method are obviously applicable. The conclusion to be reached must be constantly kept in mind, or there is a likelihood of some other conclusion being established and of the fallacy *ignoratio elenchi* being committed (*see* § 188). And every consideration which bears on that conclusion must be examined and its consequences deduced from it.

167. Special Rules.

Statement of Special Rules—

It is evident that the successful employment of the synthetic method depends upon the principles from which it starts being indisputable and clearly apprehended, and upon the reasonings employed being perfectly demonstrative, as well as arranged in natural sequence. These requirements are summed up in eight special rules, which are thus given by the Port Royalists (*op. cit.*, pp. 346-7) :—

(*a*) of Definition.

" *Two Rules touching Definitions.*

"**Rule I.** Not to leave any terms at all obscure or "equivocal, without defining them.

"**Rule II.** To employ in definitions only terms per- "fectly well known, or already explained.

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(b) of Axioms.

" *Two Rules for Axioms.*

"**Rule III.** To demand as axioms only things perfectly "evident.

"**Rule IV.** To receive as evident that which requires "only a slight attention to the recognition of its "truth.

(c) of Demonstrations.

" *Two Rules for Demonstrations.*

"**Rule V.** To prove all propositions which are at all "obscure, by employing in their proof only the "definitions which have preceded, or axioms which "have been granted, or propositions which have "been already demonstrated.

"**Rule VI.** Always to avoid the equivocation of terms, "by substituting mentally the definitions which "restrict and explain their meaning.

(d) of Method.

" *Two Rules for Method.*

"**Rule VII.** To treat of things, as far as possible, in "their natural order, by commencing with the "most general and simple, and explaining every-"thing which belongs to the nature of the genus "before passing to its particular species.

"**Rule VIII.** To divide, as far as possible, every "genus into all its species, every whole into all its "parts, and every difficulty into all its cases."

Of these rules, those on Definition have been already dealt with in an earlier part of our enquiry (*see* Bk. I , ch. v.), and the danger of neglecting them will be made yet more fully apparent in the next Book (*see* § 171).

Leibniz's analysis of the character of knowledge—

In considering the rules relating both to definitions and to axioms it is essential to have an exact idea of what is meant by knowledge being "clear" and "evident." This cannot be better obtained than by considering some passages from Leibniz's classic tract, *Reflections touching Knowledge, Truth, and Ideas.* In this he says: "Knowledge . . . is "either *obscure* or *clear.* The clear is again either *confused* "or *distinct;* and the distinct either *adequate* or *inadequate;*

"is further either *symbolical* or *intuitive;* and if it be at the "same time both adequate and intuitive, it is *perfect.*

(1) obscure, "A notion is *obscure* when it is not sufficient to enable "us to recognize the thing which it represents:—when, for "example, I remember some flower or animal which I have "formerly seen, but this remembrance is not sufficient to "enable me to recognize its image, or to discriminate it from "others which resemble it. . . .

(2) clear, "Knowledge is *clear*, on the contrary, when it enables "us to recognize the thing represented, and this [the clear] "is again either confused or distinct.

(3) confused, "It is *confused* when we are not able to enumerate marks "sufficient to discriminate the thing from others, although "it may, in reality, have such marks and requisites into "which its notion may be resolved. Thus . . . we see that "painters and other artists discern well enough what is "well or ill done; but often are not able to give a reason "for their judgment, and reply to those who enquire what "it is that displeases them in the work, that there is some-"thing, they *know not what*, wanting.

(4) distinct, "But a *distinct* notion is such as the assayers have con-"cerning gold, by marks and tests which are sufficient to "distinguish it from all other similar bodies. . . .

"A *distinct* knowledge of an indefinable notion is, how-"ever, possible when it is primitive or self-evident,—that "is, when it is ultimate. Such a notion can only be under-"stood *per se*, and thus wants the requisite of a definition. (5) inadequate, "But in composite notions, inasmuch as the individual "component marks are known—sometimes clearly indeed, "but nevertheless confusedly—such as weight, colour, "[insolubility in] aqua-fortis, and others, which enter into "the notion of gold,—such a knowledge as this of gold, "though it be distinct, is nevertheless *inadequate.*

(6) adequate, "Again, when everything which enters into a distinct "notion is distinctly known, or when the last analysis is "reached, the knowledge is *adequate*, of which I scarcely "know whether a perfect example can be offered—the "knowledge of numbers, however, approaches near to it.

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(7) symbolical,

"But for the most part, especially in longer analyses, we "do not behold at a glance the whole nature of the thing, "but employ *signs* instead of *things*. We commonly omit, "for the sake of expedition, any explication of these signs "in present thought, knowing or believing that we have "such explication in our power. Thus, when I think of a "chiliogon, or a polygon of a thousand equal sides, I do not "always expressly consider the nature of a *side*, of *equality*, "and of a *thousand* (or cube of ten); but I employ these "words—whose meaning is only obscurely and imperfectly "perceived by the mind at once—in place of the ideas which "I have concerning them, remembering that I possess the "signification of the words, but do not judge the explication "to be necessary just now. Such knowledge I am accus- "tomed to call *dark* or *symbolical*, which is the kind that "we employ in algebra and arithmetic, and indeed in almost "everything. For truly, when a notion is very complex, "we are not able to think together at once all the notions

(8) intuitive.

"which make it up. When, however, we are able wholly, "or at least to a great extent, to do this, I call the know- "ledge *intuitive*. Our knowledge of distinct primitive "notions is always simply *intuitive*, while that of composite "ones is, for the most part, only *symbolical*" (trans. by Dr. Baynes, App. to *Port Royal Logic*, pp. 424-6).

Synthesis must start from clear, distinct and adequate knowledge.

The rules for definitions and axioms, then, demand that the notions represented by them shall be clear, distinct, and adequate, whilst the fifth and sixth rules demand that we shall be always ready to make symbolical knowledge intuitive. Axioms, indeed, from their very nature as propositions whose truth is self-evident immediately the terms in which they are expressed are clearly and distinctly understood, are always cases of intuitive knowledge, in the sense in which Leibniz uses the word. Their self-evidence, therefore, is not something given by sense experience, but is due to the clear grasp in thought of the relation expressed. One particular and most important case of infringement of the fifth rule is the fallacy *petitio principii*, which will be considered in the next Book (*see* § 187).

The seventh and eighth rules are, it will be seen, nothing but mere definite and specialized statements of the four general rules of Descartes. It is, therefore, unnecessary to add to the discussion we have already given of them (*see* § 164).

168. Some Defects of Method.

The obvious should not be demonstrated.

We will close our short exposition of the principles of method by noticing some common defects which yet do not involve fallacy. One of these is to prove the obvious. "It "is this which led Euclid to prove that *the two sides of a* "*triangle are together greater than the third*, although this was "evident from the very notion of a right line, which is the "shortest possible distance between two points, and the "natural measure of the distance from one point to another, "which it would not be if it were not also the shortest of all "lines which could be drawn from one point to another" (*Port Royal Logic*, pp. 338-9). This defect is, certainly, an error on the right side; it is better to prove that which requires no proof, than to leave undemonstrated that which is not really obvious. But it is an error in method, nevertheless; it wastes time and energy, obscures the real nature of the proposition dealt with, and frequently leads to an inversion of natural order.

The natural order should be observed.

Such an inversion, indeed, is a very common defect of method, and many examples of it may be found in Euclid. "After having treated of *extension* in the first four Books, "he treats generally of the proportions of *all kinds of magni-* "*tudes* in the Fifth. He returns to *extension* in the Sixth, "and treats of *numbers* in the Seventh, Eighth, and Ninth, "and begins in the Tenth to speak again of *extension*. So "much for the general disorder; but it is full of a mass of "confusion in detail. He commences the First Book by the "construction of an equilateral triangle, and, subsequently "(twenty-two propositions after), he gives the general means "for making any triangle of three given straight lines, pro- "vided that two are greater than a single one, which involves "the particular construction of an equilateral triangle on a

"given line. . . . It would be necessary to transcribe the "whole of Euclid, in order to give all the examples which "might be found of this confusion" (*op. cit.*, p. 342).

The proof should be the simplest possible.

Another defect in method is to use a more complex demonstration when a simpler one is available. As examples of this the Port Royalists cite the fifth and forty-seventh propositions of the First Book of Euclid. The simpler and more direct the proof in every case, the more satisfactory it is.

Positive proof is always preferable to a *reductio ad impossibile.*

Lastly, it may be pointed out that just as a negative definition should never be resorted to when a positive one is possible, so a negative proof is never as satisfactory as a positive one. The *reductio ad impossibile* is necessary in some cases, but it simply establishes the conclusion as true in fact, it does not furnish any real and positive ground why it is so. The Principle of Sufficient Reason is, therefore, not satisfied, and knowledge of the essential relations of reality has not been increased. Proofs of this kind are most suitable when the proposition to be established is a negative corollary from a proposition which has been established by a positive proof, and "then that kind of demonstration, by reducing them to "the impossible, occupies the place rather of an explanation "than a new demonstration. We may say, in fine, that these "demonstrations are allowable only when we are unable to "furnish others, and that it is a fault to employ them in "proving what may be proved positively" (*op. cit.*, p. 340).

BOOK VII.

FALLACIES.

CHAPTER I.

FALLACIES IN GENERAL.

169. Nature of Fallacy.

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'Fallacy' should be restricted to violations of logical principles.

The word Fallacy is, like many other words, very loosely used in the common speech of the present day. Thus, any false statement is, by some persons, included under the term; "that men are in the habit of walking on their heads, they "would say is a very obvious fallacy" (De Morgan, *Formal Logic*, p. 238). Others include under it any false belief, or any mental confusion whatever, no matter what its origin. It seems preferable, however, to use the term in a more restricted and definite manner. It is better not to call a prejudice or a mere inaccurate statement, a fallacy, but to confine the term to offences against logical principles. We will, therefore, give as our definition

Fallacy = a violation of logical principle.

A Fallacy is a violation of logical principle disguised under a show of validity.

Wherever there is a logical principle there is a possibility of offending against it. There are, thus, fallacies incident to conception, including invalid definition and division, to judgment, to each valid form of inference, and to method. It

must be remembered, however, that the difference between these processes is not a fundamental one (*cf.* § 8); the distinction is rather between what elements in the process of attaining knowledge are explicit and what are implicit in each process. We have seen that the essence of inference is implicit in judgment, even as judgment is implicit in conception, and is involved in definition and classification (*cf.* §§ 8, 49, 54). All violations of logical principles, therefore, involve faulty inference, either implicitly or explicitly, and when those in which this fault is implicit are employed in fully stated arguments they lead to further violations of the rules of explicit inference.

A *Paralogism* is an offence against the formal rules of inference.

The words *Paralogism* and *Sophism* are also used to denote offences against logical principles. These were distinguished by Kant on purely psychological grounds. A paralogism, according to him, is a fallacy which deceives him who employs it, whilst a sophism is one which only deceives him to whom it is addressed. But such a distinction has no place in logic. If the terms are used at all, it is better to confine Paralogism to fallacies in which some formal rule of inference is openly violated; as, *e.g.*, in simple conversion of an **A** proposition, or in illicit process in syllogistic inference. This is in close accord with the use of Aristotle, and agrees with that of De Morgan, who says: "*Paralogism*, by its etymology, is best fitted to specify an "offence against the formal rules of inference" (*ibid.*, p. 239).

The word *Sophism* generally implies intent to deceive.

Sophisms were treated by Aristotle as dialectic traps laid by dishonest reasoners and litigants to catch opponents, and this implication of intentional deceit has clung to the word. But with the intention of the employer of an argument logic has nothing to do; it is the character of the argument itself which is of importance. It is, therefore, better to confine ourselves to the word Fallacy, which has no such subjective and irrelevant suggestion about it.

A *Paradox* is a very singular and improbable opinion.

Another word sometimes used as synonymous—or nearly so—with Fallacy is Paradox. But this is undesirable. "A "*Paradox* is properly something which is contrary to general "opinion: but it is frequently used to signify something

"self-contradictory. . . . The modern use of the word implies disrespect, but it was not so formerly. Thus, in the "sixteenth century the opinion of the earth's motion was "styled the *paradox of Copernicus* by writers who neither "meant praise nor blame, but only reference to the opinion "of Copernicus as an *unusual one.* The more precise writers "of our day use the word paradox for an opinion so very "singular and improbable, that the holder of it is chargeable "with an undue bias in favour of singularity or improbability for its own sake" (De Morgan, *ibid.*, pp. 338-9).

The doctrin of fallacies marks off the boundaries of valid inference.

The questions which will come up for our examination in this Book will, of course, not be new ones. In considering the conditions of valid inference, we have of necessity—in some cases explicitly, and in others by implication—dealt with invalid inference, for an invalid inference is simply one which does not fulfil those conditions. However, a separate treatment of fallacies has been customary since the time of Aristotle, and certain technical names have become traditional in this part of our subject, which could not conveniently be brought in during our positive discussion of valid inference. It has, therefore, been judged better to adhere to the traditional plan, especially as a consideration directed mainly to the chief and most common kinds of error will serve to make clearer and more definite the boundaries of that domain of valid thought which is the true province of logic.

Violations of the formal rules of inference are not uncommon.

But, even if this be granted, it may be thought that no special treatment of paralogisms—or fallacies which bear their violation of logical rule on their very face—is necessary. Aristotle, indeed, gave no separate consideration to this subject, and in this he was followed by the scholastics. To them, thoroughly familiar as they were with the rules of formal logic, an offence against those rules—except, indeed, as a pure accident, such as a mathematician might fall into in an arithmetical process—would seem impossible. But the condition of things now-a-days is very different, and we cannot better express the result on current thought than in the words of De Morgan. "We," he says, "live

"in an age in which formal logic has long been nearly "banished from education. . . . The philosophers who made "the discovery (or what has been allowed to pass for one) "that Bacon invented a new species of logic which was to "supersede that of Aristotle, and their followers, have suc-"ceeded by false history and falser theory, in driving out "from our system all study of the connexion between "thought and language. The growth of inaccurate expres-"sion which this has produced gives us swarms of legis-"lators, preachers and teachers of all kinds, who can only "deal with their own meaning as bad spellers deal with a "hard word, put together letters which give a certain resem-"blance, more or less as the case may be. Hence, what "have been aptly called 'the slipshod judgments and "crippled arguments which everyday talkers are content to "use.' Offences against the laws of syllogism (which are "all laws of common sense) are as common as any species "of fallacy: not that they are always offences in the "speaker's or writer's mind, but that they frequently "originate in his attempt to speak his mind. And the "excuse is, that he meant differently from what he said: "which is received because no one can throw the first stone "at it, but which in the middle ages would have been "regarded as a plea of guilty" (*ibid.*, pp. 240-1).

170. Classification of Fallacies.

A scientific classification of fallacies is unattainable,

A thoroughly satisfactory classification of fallacies is scarcely to be looked for. "There *is*," says De Morgan, "no such thing as a classification of the ways in which "men may arrive at an error: it is much to be doubted "whether there ever *can be*" (*ibid.*, p. 237). And, even when we limit the word Fallacy to offences against logical principles, yet it is often difficult, or even impossible, to say in any particular case, which particular rule is broken. As is well said by Whately: "From the elliptical form in "which all reasoning is usually expressed, and the peculiarly "involved and oblique form in which fallacy is for the "most part conveyed, it must of course be often a matter

"of doubt, or rather, of arbitrary *choice*, not only to which "genus each *kind* of Fallacy should be referred, but even "to which kind to refer any *one individual* Fallacy. For, "since, in any argument, one premise is usually suppressed, "it frequently happens, in the case of a Fallacy, that the "hearers are left to the alternative of supplying *either* a "premise which is *not true*, or *else*, one which *does not prove* "the conclusion, *e.g.*, if a man expatiates on the distress of "the country, and thence argues that the Government is "tyrannical, we must suppose him to assume *either* that "'every distressed country is under a tyranny,' which is a "manifest falsehood, *or*, merely that 'every country which "is under a tyranny is distressed,' which, however true, "proves nothing, the middle term being undistributed" (*Elements of Logic*, pp. 170-1).

and a particular fallacy can often be referred to more than one head.

It must not, therefore, be expected that a rigidly scientific classification of fallacies can be drawn out. We will now briefly describe some of the most noteworthy attempts which have been made in addition to that of Bacon, with which we have already dealt [*see* § 145 (iii.)].

(i.) Aristotle's Classification.

Aristotle's Classification—

Aristotle, as we have already said, only treated separately of sophisms, or sham arguments whereby an unfair controversialist endeavoured to refute the reasonings urged against him by his opponent. Of these he made two classes :—

1. *Sophismata in dictione.*

1. *Sophismata in dictione*, or fallacies arising directly out of the ambiguous use of language. Of these he enumerated six species—

(*a*) *Æquivocatio*—due to ambiguity in a single term.
(*b*) *Amphibolia*—due to ambiguity in the construction of a sentence.
(*c*) *Compositio* }—due to confusion between the collec-
(*d*) *Divisio* } tive and distributive use of a term.
(*e*) *Accentus*—due to ambiguity of emphasis.
(*f*) *Figura dictionis*—due to ambiguity arising from the metaphorical use of words.

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2. Sophismata extra dictionem.

2. *Sophismata extra dictionem*, or those whose fault can only be detected by an examination of the matter dealt with. Of these Aristotle gave seven kinds—

(*a*) *Accidens*—due to equating subject and accident, *i.e.*, genus and species, or species and individual contained under it.

(*b*) *Fallacia a dicto secundum quid ad dictum simpliciter* and its converse—due to confusing a statement made with some limitation with one made absolutely.

(*c*) *Ignoratio elenchi*—or refuting the wrong point.

(*d*) *Consequens*—due to simple conversion of a hypothetical proposition.

(*e*) *Petitio principii*—or the assumption without proof of a proposition requiring proof.

(*f*) *Non causa pro causa*—or deducing a conclusion from premises which do not necessitate it.

(*g*) *Plures interrogationes*—or the effort to obtain a single answer to several questions asked as one.

This classification was adhered to by the scholastic logicians, and is still followed by some good writers on the subject.

* (ii.) Whately's Classification.

Whately's Classification—

Whately objected to Aristotle's classification as not "grounded on any distinct principle" (*op. cit.*, p. 169), and himself gave the following arrangement which has been frequently followed in English books on logic.

1. Logical Fallacies:

1. *Logical Fallacies*—"where the conclusion does not follow from the premises" (*op. cit.*, p. 172), including

(a) Purely Logical.

(*a*) *Purely Logical*, "as exhibiting their fallaciousness "by the bare *form* of the expression, without "any regard to the meaning of the terms." Under this head come all obvious violations of the syllogistic rules (*see* § 111[1]).

(b) Semi-Logical.

b) *Semi-logical Fallacies*, "viz., all the cases of "ambiguous middle term except its non-dis-

[1] First Edition, § 125.

"tribution." This class includes all Aristotle's Sophisms except the three mentioned under the next head.

2. *Material or Non-logical Fallacies*—"where the con-"clusion does follow from the premises," including *Ignoratio elenchi*, and *Petitio principii*, the latter including as a sub-class, *Non causa pro causa*. 2. Material Fallacies.

The nomenclature here is due to Whately's nominalistic conception of Logic, the sole province of which is, he says, "to ascertain the validity or fallaciousness of any apparent "argument, as far as the *form of expression* is concerned" (*ibid.*, p. 166). All fallacies are 'logical' in the sense of involving an offence against logical principles; though as invalid inferences, they would be more commonly called 'illogical.' The subdivision of 'Logical Fallacies' into 'Purely Logical' and 'Semi-Logical' seems to be incapable of valid justification, as exactly the same rules of inference are violated in each class. From Whately's own standpoint, 'Non-Logical Fallacies' can have no place in logic at all, and consistency would have required their exclusion.

(iii.) Mill's Classification.

Mill, with his wide view of the scope of Logic, included under fallacies all sources and kinds of intellectual error; "A catalogue of the varieties of apparent evidence which "are not real evidence," says he, "is an enumeration of "Fallacies" (*Logic*, V., i., § 1). He makes a primary division into *Fallacies of Simple Inspection* or the *à priori* assumption of propositions without proof or reasoning of any kind, and *Fallacies of Inference*. The latter class is then subdivided on the basis of whether the evidence is, or is not, distinctly conceived. In the former case there are Inductive and Deductive Fallacies, and in either of these the fallacy may be due either to the assumption of false premises or to the fact that the premises, though true, do not support the conclusion. This gives in Induction, *Fallacies of Observation* and *Fallacies of Generalization*. The assumption of false

Mill's Classification—
1. Fallacies of Simple Inspection.
2. Fallacies of Inference.
(*a*) Inductive
(i.) in Observation,
(ii.) in Generalization.
(*b*) Deductive.
(*c*) of Confusion.

premises, however, in a deductive argument is reducible to either an inductive fallacy, or an *à priori* fallacy. Consequently, we here have only one class—*Fallacies of Ratiocination*. There are thus left three distinct classes of fallacies of inference when the evidence is distinctly conceived. Thus, ultimately a five-fold classification is attained :—

1. *Fallacies of Simple Inspection*—the improper acceptance of propositions as self-evident.
2. *Fallacies of Observation* "of which the error lies in not "sufficiently ascertaining the facts on which the theory "is grounded; whether the cause of failure be mal-"observation, or simple non-observation, and whether "the mal-observation be direct, or by means of inter-"mediate marks which do not prove what they are "supposed to prove" (*ibid.*, V., ii., § 2).
3. *Fallacies of Generalization*—due to misconception of "the legitimate mode of drawing conclusions from "observation and experiment" (*ibid.*, V., v., § 1). This class of fallacies "is the most extensive of all," and an exhaustive classification is "chimerical" (*ibid.*).
4. *Fallacies of Ratiocination*, or offences against the formal rules of inference. Under this head come Whately's 'Purely Logical' Fallacies, and the fallacy *à dicto secundum quid ad dictum simpliciter* and its converse.
5. *Fallacies of Confusion*, "in which the source of error is "not so much a false estimate of the probative force "of known evidence, as an indistinct, indefinite, and "fluctuating conception of what the evidence is" (*ibid.*, V., vii., § 1). Under this head come Whately's 'Semi-Logical' and 'Non-Logical' Fallacies; *i.e.*, all fallacies due to ambiguities in language, to begging the question, and to missing the point at issue.

This classification is based rather on a consideration of the source of the fallacy than on an examination of its nature. It has, however, the merits of fulness and comprehensiveness,

and of being based on an intelligible principle, though we do not think that principle the most convenient one. BOOK VII. Ch. I.

(iv.) Classification according to Logical Principle violated.

The most convenient classification of fallacies seems to us to be one based on the logical principle violated in each case. We shall thus consider in order fallacies incident to each of the divisions of logical doctrine which have occupied us in the preceding six Books. It will be found that all the traditional fallacies will find a place in such a classification. Our classification stands as follows :—

The most convenient classification is based on the logical principle violated.

This gives Fallacies incident to—

A. Fallacies incident to Conception.

1. Conception.

1. Faulty, or imperfectly conceived, Definition.
   - (*a*) Embracing incompatible attributes.
   - (*b*) Aristotle's *Æquivocatio.*
   - (*c*) „ *Figura dictionis.*
   - (*d*) „ *a dicto secundum quid ad dictum simpliciter* and its converse.
   - (*e*) „ *Compositio* and *Divisio.*
2. Faulty Division.
   - (*a*) Change of *fundamentum divisionis.*
   - (*b*) Non-exhaustive division.
   - (*c*) Omission of steps in division.

B. Fallacies incident to Judgment.

2. Judgment.

1. Judgment involving self-contradiction.
2. Misinterpretation of categorical propositions.
   - (*a*) Aristotle's *Amphibolia.*
   - (*b*) „ *Accentus.*
3. Misinterpretation of hypothetical propositions.
4. Misinterpretation of disjunctive propositions.

C. Fallacies incident to Immediate Inference.

3. Immediate Inference.

1. False Opposition, including Aristotle's *Plures interrogationes.*

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2. Illicit Conversion.
    (*a*) Of an **A** or **O** proposition.
    (*b*) Aristotle's *Accidens*.
    (*c*) " *Consequens*.
3. Illicit Contraposition.
4. Illicit Inversion.

4. Deductive Inference.

D. Fallacies incident to Deductive Inference.

1. Abstract.
    (*a*) Undistributed Middle.
    (*b*) Illicit Process of the Major.
    (*c*) Illicit Process of the Minor.
2. Concrete—Four Terms—including (*a*) the use of a proposition involving any of the fallacies under A, 1. (*b*), (*c*), (*d*), (*e*), and B, 2.

5. Inductive Inference.

E. Fallacies incident to Inductive Inference.

1. False Analogy, leading to wrong hypothesis.
2. Imperfect Observation.
3. Illicit Generalization.

6. Method.

F. Fallacies incident to Method.

1. Taking as axioms propositions which are not self-evident.
2. Aristotle's *Petitio principii*.
3. " *Ignoratio elenchi*.
4. " *Non causa pro causa*.

CHAPTER II.

FALLACIES INCIDENT TO CONCEPTION.

171. Faults in Definition.

All fallacies due to ambiguity of terms are, at bottom, faults of definition.

Any violation of the rules of definition (*see* § 52[1]) leads to fallacy through a faulty or imperfect conception of the force of the terms employed. Thought wanting in clearness and definiteness is extremely likely to become, at some point or other, actually erroneous. All fallacies due to any kind of ambiguity in the terms employed are, thus, at bottom faults of definition; once make the meaning of every term employed clear and such fallacies become impossible. Under the general head of faulty definition we may include several of the traditional Aristotelian fallacies, as well as one which is specially incident to vague and indefinite conception and which we will consider first.

(i.) Concept embracing incompatible attributes.

When the content of a concept is not made definite and explicit, it may embrace incompatible attributes.

Whenever the meaning of a term is explicitly and clearly conceived it is, of course, impossible to embrace in it incompatible attributes. But when one's ideas are not clearly and definitely set forth, this is by no means impossible. Thus we find people arguing about 'an indivisible portion of matter,' where the attribute 'indivisible' is incompatible with 'matter' which implies extension, and therefore, divisibility. The concept of 'indivisible matter' is, therefore, self-contradictory. All the numerous and varied attempts which have been made at 'squaring the circle' may be set down to a faulty conception of 'incommensurable,' a conception which,

[1] First Edition, § 59.

indeed, embraces the idea of 'commensurable in terms of an indefinitely small unit.' Similarly the word 'infinite' seems by many people to be held to mean 'finite at a very extended limit,' and 'eternal' to signify 'enduring throughout a very long time,' whereas the very notion of time, embracing, as it does, the ideas of before and after, and consequently of beginning and end, is incompatible with that of 'eternity,' whose true meaning is existence out of time.

(ii.) Æquivocatio or Homonymia.

Equivocation = use of ambiguous term.

By a fallacy of equivocation is meant one due to the use of a word capable of two or more meanings. Traced to its origin, this implies that the concept corresponding to the word is wanting in clearness and accuracy. In other words it is a failure in definition. Such failure may, of course, be in the mind of him who uses the ambiguous term, or its use may be merely a sophistical device, the person who uses the term in two different senses trusting that the change of meaning may escape detection. When such a word is employed in a syllogistic argument, we have really four terms, and consequently the syllogism is only an apparent one. The ambiguity is generally in the middle term. Of this we gave an example in discussing the rules of the syllogism [*see* § 111 (ii.)[1]]. An example commonly given by the old logicians is—Finis rei est illius perfectio, Mors est finis vitæ, Ergo, Mors est perfectio vitæ. Here, as De Morgan points out, the ambiguity may be thrown either on 'finis' or on 'perfectio,' giving in the former case ambiguous middle, and in the latter ambiguous major. Similarly in 'Knowledge is power, Perception is knowledge, Therefore, perception is power,' we have an ambiguous use of the term knowledge. In such examples as these the error is obvious and, consequently, not likely to be committed. But it must be remembered that a writer on fallacies is bound to choose some, at least, of his examples of such a character that every reader will see at once where the fallacy comes in. Moreover, when an argument is, as it were, reduced to its lowest terms, and stated in strict syllogistic form, it is easy to see an error, which might well

Such fallacies most frequently occur in an extended discourse.

[1] First Edition, § 125 (ii.).

escape detection if it occurred in a long and more or less involved disquisition, with the premises, perhaps, far apart. Fallacies due to ambiguity of words are, indeed, amongst those most commonly committed. As Bacon well says: "Men believe that their reason rules over words; but it is "also the case that words react, and in their turn use their "influence on the intellect" (*Nov. Org.*, I. 59).

Gradual change of meaning may lead to this fallacy

The chief ways in which such ambiguity originates were indicated in § 3,[1] and it was there seen that one of the chief sources of ambiguity is that a word which has changed its meaning still retains some of the associations which were connected with its original meaning. De Morgan gives some good examples, one or two of which we will borrow. "The word *publication* has gradually changed its meaning, "except in the courts of law. It stood for *communication to* "*others* without reference to the mode of communication, "or the number of recipients. Gradually, as printing be-"came the easiest and most usual mode of publication, and "consequently the one most frequently resorted to, the word "acquired its modern meaning; if we say a man publishes "his travels, we mean that he writes and prints a book "descriptive of them. I suspect that many persons have "come within the danger of the law, by not knowing that "to write a letter which contains defamation, and to send it "to another person to read, is *publishing a libel;* that is, by "imagining that they were safe from the consequences of "publishing, as long as they did not print. . . . A similar "change has taken place in the meaning of the word to "*utter*, the sense of which is to *give out*, but which now "means usually to give out of the mouth in words. As yet "I am not aware that any person charged with the *utterance* "of counterfeit coin has pleaded that no one ever uttered "coin except the princess in the fairy tale: but there is no "saying to what we may come, with good example, and "under high authority" (*Formal Logic*, p. 243). Confusion between the etymological and the currently accepted meaning of a word is, indeed, always liable to lead to fallacy. In truth, in a discussion as to the meaning of a word, an

[1] First Edition, § 7.

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appeal to etymology is out of court; current usage alone can decide the question.

Clear terms are essential to scientific thought.

In considering the question of a scientific nomenclature and terminology, we pointed out how important for exact thought is an exact system of verbal symbols (*see* §§ 64, 65[1]). The moral sciences have no such well-established and exact system of technical terms, and it follows that in those sciences fallacies due to ambiguity are most easily fallen into. Especially is this the case with Economics. Mill gives a good example of such a fallacy which, in fact, has been and still is by no means infrequently committed. He says:

Examples of ambiguous terms—

1. 'Money.'

"The mercantile public are frequently led into this fallacy "by the phrase, 'scarcity of money.' In the language of "commerce 'money' has two meanings: *currency*, or the "circulating medium; and *capital seeking investment*, especi- "ally investment on loan. In this last sense the word is "used when the 'money market' is spoken of, and when the "'value of money' is said to be high or low, the rate of "interest being meant. The consequence of this ambiguity "is, that as soon as scarcity of money in the latter of these "senses begins to be felt—as soon as there is difficulty of "obtaining loans, and the rate of interest is high—it is con- "cluded that this must arise from causes acting upon the "quantity of money in the other and more popular sense; "that the circulating medium must have diminished in "quantity, or ought to be increased. I am aware that, inde- "pendently of the double meaning of the term, there are in "the facts themselves some peculiarities, giving an apparent "support to this error; but the ambiguity of the language "stands on the very threshold of the subject, and intercepts "all attempts to throw light upon it" (*Logic*, V., vii., § 1).

2. 'Government.'

Another ambiguous word is 'government,' which is used both to denote the system of laws established in a nation, and the body of men charged with the carrying out of those laws. Loyalty to the government in the former sense may involve resistance to the government in the latter sense, though a tyranny would not be willing to acknowledge this, and would trade on the ambiguity of the word, trusting that

[1] First Edition, §§ 71, 72.

the ignominy which may rightly accrue to resistance to law will attach to resistance to the men whose mal-administration of the law may have become intolerable.

'Nature.'

'Nature' is another fertile source of fallacy. What, for instance, is 'education according to nature'? With Comenius it meant seeking to base educational method on fanciful analogies drawn from the physical world, as, for instance, when he argues that because the sun does not occupy himself with objects one by one—a tree or an animal—but illumines and warms the whole earth, therefore, there should be only one teacher for each class (*see* Laurie's *Comenius*, p. 104). With Rousseau the same phrase meant a return as far as possible to the condition of uncivilized man, 'nature' being regarded as the condition from which man starts. The same idea is present in the excuse so often given for childish faults that they are 'natural.' On the other hand, Plato and all modern idealists would seek man's true nature in the ideal towards which all civilization can only be regarded as a slow progress, and with them 'education according to nature' means guiding the child in the path of this development in the order and to the extent to which a study of psychology shows to be possible.

Equivocation may be due to want of indication that a word is used in a special sense.

One frequent cause of the fallacy of equivocation is when a writer or speaker uses a word in a definite and perhaps special meaning without taking steps to secure that it is understood in that meaning. "It is very difficult," says De Morgan, "to avoid this form of the fallacy, without "giving the meaning of the most essential terms, on the first "occasions of their occurrence. It is not uncommon to meet "with a writer who appears to believe, at least who certainly "acts upon, the notion that the right over words resides in "him, and that others are wrong as far as they differ from "him. . . . The writers of whom I speak . . . treat words "as absolute images of things by right of the letters which "spell them. 'The French,' said the sailor, 'call a cabbage "a *shoe*; the fools! why can't they call it a cabbage, when "they must know it is one?'" (*ibid.*, pp. 246-7).

This last example leads us to notice that all puns are

logically instances of this fallacy. Lamb in the *Essays of Elia* quotes the following from Swift's *Miscellanies:* "An "Oxford scholar meeting a porter who was carrying a hare "through the streets, accosts him with this extraordinary "question: 'Prithee, friend, is that thine own hare or a "wig?'" Similarly, the proof that every cat has three tails, because no cat has two tails and every cat has one more tail than no cat, turns on ambiguity of terms.

Puns are examples of *equivocatio.*

The fallacy may arise from neglect of context.

It is probably impossible to enumerate all possible sources of ambiguity, and the reader will readily suggest many others to himself. We will here only notice one more in which the ambiguity is rather in a phrase than in the separate words of that phrase. Words when united frequently have together a compound meaning which a mere union of their separate meanings will not give. That is to say, the force of a word depends partly on the context in which it occurs. Thus, "a person undertakes to cross a bridge in an "incredibly short time; and redeems his pledge by crossing "the bridge as one would cross a street, that is, by traversing "the breadth. Now, though it is true that, in general, to "cross is to go over the breadth, or shorter dimension, yet "in the case before us, the phrase is elliptical and signifies "crossing *the river* upon the bridge. Nor can it be said "that this common meaning is incorrect; that which is "common and well known is, in language, always correct" (*ibid.*, p. 246).

(iii.) Figura Dictionis.

Figure of Speech is a form of ambiguity,

The *Fallacia Figuræ Dictionis* or Fallacy of *Figure of Speech* denoted with Aristotle sophisms which might arise from supposing words similar in form to be similar in meaning; *e.g.*, that *poeta* is of the feminine gender because most Latin words with the same termination are so. The following invalid argument from Mill's Utilitarianism seems to fall under this head. "The only proof "capable of being given that an object is visible, is that "people actually see it. The only proof that a sound is "audible, is that people hear it. . . . In like manner, I

"apprehend, the sole evidence it is possible to produce that "anything is desirable, is that people do actually desire "it." Here Mill assumes that the meaning of 'desirable' is analogous to that of 'visible' and 'audible.' But 'visible' simply means 'able to be seen,' and 'audible' 'able to be heard.' But 'desirable' does not mean 'what can be desired' but 'what should be desired' (*cf.* Mackenzie, *Manual of Ethics*, pp. 98-9). The fallacy has been extended to cover other perversions of grammar, as in the traditional example: 'What a man walks on he tramples on, This man walks on the whole day, Therefore, he tramples on the whole day.' This, it will be seen is only another source of ambiguity in words, and the fallacy does not differ in essence from *equivocatio*. The most important cases under this head are those in which error arises from the use of paronyms or conjugate words, as different parts of speech derived from the same root. As Dr. Davis points out, "These have by no means "similar meanings, *e.g.*, 'Artist, artisan, artful'; 'Pity and "pitiful'; 'Presume and presumption'; 'Project and pro-"jector'; What is 'imaginary' is unreal, but an 'image' "formed of wood or stone is real; To 'apprehend' is to "lay hold on, or to come to a knowledge of, while 'appre-"'hension' often signifies fear or dread" (*Theory of Thought*, p. 270).

and arises chiefly in the use of paronymous words.

Mill instances "the popular error that *strong* drink must "be a cause of *strength*" as an example of this fallacy, and one which involves the further fallacy of supposing that an effect must be like its cause—using the words in their popular sense. People who fall into this error should, as Dr. Davis suggests, try strong poison; which is, perhaps, exactly what many ardent teetotalers would say they are really doing. This fallacy, like the last, is an offence against the rules of definition, and when employed in syllogistic argument gives rise to the fallacy of four terms.

(iv.) A dicto simpliciter ad dictum secundum quid. A dicto secundum quid ad dictum simpliciter.

These fallacies are converses of each other, and their

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A dicto . . . ad dictum confuses the absolute and limited uses of a term.

essence is the confusion of an absolute statement with one limited or qualified in some way pertinent to the matter in hand. In other words, the essence of the fallacy is a failure to definitely determine the force of our terms, *i.e.*, it is a fallacy in nominal definition [*cf.* § 53 (i.)[1]]. The old standard example of the first is: 'What you bought yesterday you ate to-day, You bought raw meat yesterday, Therefore, you ate raw meat to-day,' where the fallacy lies in not making clear that the 'rawness' is not regarded in the major premise as a relevant circumstance, and then assuming it to be relevant in the conclusion. On this example De Morgan remarks: "This piece of meat has remained un-"cooked, as fresh as ever, a prodigious time. It was raw "when Reisch mentioned it in the *Margarita Philosophica* "in 1496; and Dr. Whately found it in just the same state "in 1826" (*op. cit.*, p. 251). An example of the converse form is: 'Everything which is harmful should be forbidden, Wine is harmful, Therefore, its use should be prohibited,' where the conclusion omits the tacitly understood, but very relevant, condition in the given minor premise of immoderate use. In each case it will be seen on examination that the apparent syllogism contains four terms; either the middle being ambiguous or illicit process being committed.

Inference from one special case to a different special case is an example of this fallacy.

Under the same head must be included arguments which illicitly conclude from a statement qualified in one way to one qualified in another way. These may be called *fallacia a dicto secundum quid ad dictum secundum alterum quid.* An example would be to conclude from the assertion—whether true or not—that to take life in sport is cruel, therefore, to eat flesh from which life has been taken by others is to show a cruel disposition. This is to infer from one special case to another special case differing from it in circumstances distinctly relevant to the question of intention, which is the point under discussion. "All the fallacies "which attempt the substitution of a thing in one form for "the *same thing* (as it is called) in another, belong to this "head: such as that of the man who claimed to have "had one knife twenty years, giving it sometimes a new

[1] First Edition, § 60 (i.).

"handle, and sometimes a new blade" (De Morgan, *op. cit.*, p. 252).

An amusing example of arguing *a dicto simpliciter ad dictum secundum quid* is contained in the following story told by Boccaccio in the *Decameron*: A servant who was roasting a stork for his master was prevailed upon by his sweetheart to cut off a leg for her to eat. When the bird came upon the table, the master desired to know what had become of the other leg. The man answered that storks never had more than one leg. The master, very angry, but determined to strike his servant dumb before he punished him, took him next day into the fields where they saw some storks, standing each on one leg, as storks do. The servant turned triumphantly to his master; on which the latter shouted, and the birds put down their other legs and flew away. "Ah, sir," said the servant, "you did not shout to "the stork at dinner yesterday: if you had done so, he "would have shown his other leg too."

The fallacy is committed when a general rule is applied to a special case with no consideration of modifying circumstances.

More serious instances of the same fallacy are found in applications of abstract general rules to particular concrete cases without taking account of any modifying circumstances that may exist. Errors of this character are easily fallen into in the application of general rules of either social or individual life to special cases. Hence arise many wild assertions on economic and social questions. The argument, for instance, that because employment of labour is beneficial to the community, therefore, unemployed workmen may wisely be set to do work of an entirely useless character, merely to find them employment, is an example of reasoning *a dicto secundum quid ad dictum simpliciter*, as it omits in the conclusion the relevant condition without which the given premise is false, viz., that the work must be productive of some utility. So too, the application of the 'common sense of our ancestors' embodied in proverbs is very liable to involve the fallacy of arguing *a dicto simpliciter ad dictum secundum quid.* 'What man has done man may do' we are told for our encouragement; but it scarcely seems to follow that each one of us is capable of becoming a Shakespeare or a Newton.

In legal cases the *secundum quid* must be very exactly stated.

De Morgan points out that "the law, in criminal cases, "demands a degree of accuracy in the statement of the "*secundum quid* which many people think is absurd" (*op. cit.*, p. 252). He then gives two instances which he discusses at length. In the first, a man tried for stealing a ham was acquitted on the ground that what was proved against him was that he had stolen a piece of a ham. In the second, a man was committed for perjury 'in the year 1846' and the judge admitted the objection that it ought to have been 'in the year of our Lord 1846.' De Morgan argues—and it seems to us conclusively—that in the latter case the *secundum quid* is unessential, for "as things stand, "there is no imaginable difference: for there is only one "era from which we reckon" (*ibid.*, p. 254). But in the former case the difference between the two descriptions is material. For suppose that the two descriptions were put before two different persons. "One is told that a man has "stolen a ham; another that he has stolen a part of a ham. "The first will think he has robbed a provision warehouse, "and is a deliberate thief: the second may suppose that he "has pilfered from a cook-shop, possibly from hunger. As "things stand, the two descriptions may suggest different "amounts of criminality and different motives" (*ibid.*, p. 253).

(v.) Compositio and Divisio.

Composition and *Division* are due to confusion between the distributive and the collective use of terms.

The fallacies of *Composition* and *Division* are converses of each other, and are most conveniently considered together. The error consists in joining together things which ought to be kept separate, or separating those which ought to be kept conjoined. In other words, there is confusion between a collective and a universal term [*cf.* § 27 (ii.)[1]]. To take Aristotle's example: "Two and three are even and odd; "Two and three are five; Therefore, five is even and odd"; which commits the fallacy of composition. Similarly, three and five are (together) four and four (together); but neither three nor five is four; and such an inference would be

[1] First Edition, § 33 (ii.).

vitiated by the fallacy of division. Such examples are obvious, but they do not differ in essence from the argument given by Mill in support of utilitarianism. He says: "No "reason can be given why the general happiness is desirable "except that each person, as far as he believes it to be at-"tainable, desires his own happiness. This, however, being "a fact, we have not only all the proof which the case ad-"mits of, but all which it is possible to require, that happi-"ness is a good: that each person's happiness is a good to "that person, and the general happiness, therefore, a good "to the aggregate of all persons" (*Utilitarianism*, p. 53). It would be difficult, as Professor Mackenzie says, "to collect "in so short a space so many fallacies as are here committed" (*see Manual of Ethics*, 3rd. ed., p. 219). There is a fallacy of equivocation in the use of the ambiguous word 'desirable,' used by Mill in two different senses. But it is with the last part of the argument that we are here concerned. It is an obvious example of the fallacy of composition. Put symbolically it is A desires the happiness of A, B that of B, C that of C, etc., therefore A desires the happiness of $A+B+C$, so does B, and so does C, which is about equivalent to arguing in mathematics that $ax+by+cz=(a+b+c)(x+y+z)$.

The converse fallacy of division seems to lurk in many of the arguments brought forward in support of an encyclopædic curriculum for all schools. That a knowledge of this, and that, and the other subject is necessary to the community, therefore, all those subjects should be taught to each member of that community, is the line of reasoning frequently adopted, as, for example by Mr. Herbert Spencer in the First Chapter of his book on *Education*.

The fallacy often involves confusion between a disjunctive and a copulative proposition.

The most common form of the fallacy may be reduced to an implicit confusion between a disjunctive and a copulative proposition. Thus, the spendthrift, falling into the fallacy of composition, argues 'I can afford *a or b or c or* . . . *z*, therefore, I can afford *a and b and c and* . . . *z*.' On the other hand the converse fallacy of division is often found lurking in the argument by which a miserly person refuses to subscribe to any charitable object. 'I cannot afford to

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subscribe to *a and b and c and . . . z,* therefore I cannot afford to subscribe to *a or b or c or . . . z.*'

The ambiguity of 'all' frequently leads to this fallacy,

The ambiguity of the word 'all' is a frequent occasion for this fallacy [*cf.* § 27 (ii.)]. As De Morgan says: "It must "be remembered that the word *all*, in a proposition, is not "necessarily significant of a universal proposition: it may "be a part of the description of the subject. Thus, in 'all "the peers are a House of Parliament,' we do not use the "words *all the peers* in the same sense as when we say 'all "the peers derive their titles from the Crown.' In the "second case the subject of the proposition is *peer;* and the "term *all* is distributive, synonymous with each and every. "In the first case the subject is *all the peers*, and the term "*all* is collective, no more distinguishing one peer from "another than one of John's fingers is distinguished from "another in the phrase 'John is a man.' The same remarks "may be made on the word *some;* as in 'some peers are "dukes,' and 'some peers are the committee of privileges'" (*ibid.*, p. 248). It is obvious that this fallacy also formally leads to there being four terms in any apparent syllogism in which it occurs.

similarly with 'some.'

172. Faults in Division.

The violation of any rule of division involves fallacy.

A violation of any of the fundamental rules of division involves fallacy (*see* § 55[1]). Corresponding to each rule there is the fallacy involved in its violation. We may thus enumerate three classes of fallacy of division, viz. :—

(i.) Changing the *fundamentum divisionis.*
(ii.) Omitting part of the genus to be divided.
(iii.) In a continued division, not proceeding by proximate steps.

These faults have been sufficiently discussed and illustrated in our positive treatment of the rules of definition, to which we need here only refer (*see* § 55[1]).

[1] First Edition, § 62.

CHAPTER III.

FALLACIES INCIDENT TO JUDGMENT.

173. Judgment involving Self-contradiction.

Incompatibility between predicate and subject is possible when the terms are not clearly apprehended.

The fallacies incident to judgment are not essentially different from those incident to conception, as, indeed, might be anticipated from the fundamental identity of mental process underlying the two forms of thought (*cf.* § 8). In a clearly apprehended judgment there can, of course, be no incompatibility between predicate and subject. But when the force of the judgment is not clearly apprehended such incompatibility is by no means impossible. The careless use of language, which is so common nowadays, makes this particularly easy. Especially dangerous is the habit of making universal assertions when there is only justification in fact for particular ones. We may thus have such self-destructive assertions as that 'Every rule has an exception.' This statement, being itself a rule, contradicts itself. Thus we may put it syllogistically: 'Every rule has exceptions, This statement is a rule, Therefore, this statement has exceptions, *i.e.*, Some rules have no exceptions'; when the contradiction implicit in the major premise is made explicit. Similar considerations solve the old logical puzzle involved in 'Epimenides the Cretan says that all Cretans are always liars; therefore Epimenides can only speak the truth if he lies, and lies if he speaks the truth.' Here also the major is self-contradictory, for every judgment claims to be true, and this proposition at the same time asserts its own falsity. Quite similar is the scientific conundrum as to what would happen if an irresistible force were to impinge on an immovable body, where two incompatible conditions are assumed to be simultaneously operative.

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174. Misinterpretation of Categorical Propositions.

Even when the meaning of each individual word in a sentence is clearly apprehended, there is still a possibility of misunderstanding the statement as a whole, owing to some ambiguity of construction or of interpretation. This, of course, results from the dependence of terms upon the context for their full meaning. The two chief cases were noticed by Aristotle.

(i.) Amphibolia.

Amphibolia= ambiguity of construction.

The *Fallacia Amphiboliæ* originates in ambiguity due to the construction of a sentence. It is in essence a misinterpretation of a proposition. It does not, therefore, differ fundamentally from fallacies of ambiguous terms, for an ambiguous term employed in a proposition necessarily leads to that proposition also being ambiguous. However, in amphiboly the ambiguity lies in the general structure of the proposition rather than in the terms it contains. Latin with its construction of accusative with infinitive in indirect narration lent itself very readily to this form of ambiguity. Thus, the oracle given to Pyrrhus : "Aio te, Æacida, "Romanos vincere posse"—"Pyrrhus the Romans shall, I "say, subdue"—left it entirely in doubt on which side victory was to lie. This is very similar, as Shakespeare makes the Duke of York point out, to the witch's prophecy in Henry VI.—"The Duke yet lives that Henry shall depose." (*Second Part*, Act i., sc. 4). Open in the same way to double interpretation is the line in W. R. Spencer's poem *Gelert :* "The noble hound the wolf hath slain." The walls of Windsor Castle still bear the ambiguous sentence "Hoc fecit Wykeham," whose capability of a double reading is said to have averted the King's displeasure from the bishop, who, like a true courtier, explained it as meaning that the tower made him.

Amphiboly arises from careless sequence of words.

One has not far to seek to find numerous instances of amphiboly in modern English, generally due to want of precision in the order in which the words are arranged, a point of great importance in an analytic language. A few examples

may be given: 'Locke was an unquestioned man of genius'; 'Please to receive a ticket from the attendant torn from the book'; 'An exhibition of drawings by lady amateurs well worthy of inspection'; 'Wolsey left at his death many buildings which he had begun in an unfinished state.' The newspapers often furnish a happy hunting-ground for the logician in search of examples of this fallacy. We will cull a few flowers of rhetoric of this order—'There is an odd little story of Madame Hading drifting about New York'; 'Lost, a valuable silk umbrella belonging to a gentleman with a curiously carved head'; 'A piano for sale by a lady about to cross the Channel in an oak case with carved legs'; 'Wanted, a handsome Shetland pony for a child with a long mane and tail'; 'M. J. S. was charged with breaking two panes of glass at the house of her husband from whom she has been recently separated in the Cambridge Road'; 'Mr. Disraeli delivered a rambling and disjointed string of jocosities and abstractions, by no means equal to his last Irish speech which rather wearied the House'; 'Lord Salisbury will reply to Mr. Gladstone's recent Birmingham speech at the Guildhall.'[1]

Ambiguity may arise from making an affirmation in the form of a qualified negative.

A subtle form of amphiboly, and one not uncommon in controversial writings is instanced by De Morgan: "Equi-"vocation may be used in the form of a proposition; as for "instance, in throwing what ought to be an affirmative into "the form of a qualified negative, with the view of making "the negative form produce an impression. Thus a contro-"versial writer will assert that his opponent has not attempted "to touch a certain point, except by the absurd assertion, "etc., etc., etc. To which the other party might justly "reply: 'Your own words show that I have made the "attempt, though your phrase has a tendency, perhaps in-"tended, to make your reader think that there is none, or at "least to blind him to the difference between *none* and *none* "*that you approve of*'" (*ibid.*, p. 247).

[1] The reader will find a copious selection of amusing examples in Mr. Salmon's little book on *School Composition*, from which many of those quoted in the text have been borrowed.

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(ii.) Accentus.

Accentus= fallacy of false stress.

The *Fallacia Accentus* or *Prosodiæ* was, with Aristotle, due to the fact that in Greek the same word, differently accentuated had a different meaning. In the earlier times Greek writing was devoid of accents and breathings, and hence written words were sometimes ambiguous which when spoken were not so. Aristotle consequently remarks that this ambiguity can scarcely occur in speech. But with us the fallacy may be committed by means of false stress either in speech or in writing. Thus, as De Morgan points out, the commandment, 'Thou shalt not bear false witness against thy neighbour,' may be read "either so as to convey "the opposite of a prohibition, or to suggest that subor-"nation is not forbidden, or that anything false except "evidence is permitted, or that it may be given *for* him, or "that it is only against *neighbours* that false witness may "not be borne" (*ibid.*, p. 249). Other ways in which this fallacy may be fallen into are thus summed up by the same writer: "A statement of what was said with the suppres-"sion of such tone as was meant to accompany it, is the "*fallacia accentus.* Gesture and manner often make the "difference between irony and sarcasm, and ordinary asser-"tion. A person who quotes another, omitting anything "which serves to show the *animus* of the meaning; or one "who without notice puts any word of the author he cites "in italics, so as to alter its emphasis; or one who attempts "to heighten his own assertions, so as to make them imply "more than he would openly say, by italics, or notes of ex-"clamation, or otherwise, is guilty of the *fallacia accentus*. . . . "I may here observe that irony . . . is generally accompanied "by the *fallacia accentus;* perhaps cannot be assumed with-"out it. A writer disclaims attempting a certain task as "above his powers, or doubts about deciding a proposition "as beyond his knowledge. A self-sufficient opponent is "very effective in assuring him that his diffidence is highly "commendable, and fully justified by the circumstances" (*ibid.*, pp. 249-50).

175. Misinterpretation of Hypothetical Propositions.

Fallacy may arise from regarding the protasis as stating a causa essendi.

The characteristic fallacy of misinterpretation of a hypothetical proposition is to assume that the protasis contains the *causa essendi*, and not simply the *causa cognoscendi* of the apodosis. As has been already pointed out, the latter may be a mere sign of the former or may actually be its effect (*see* § 144). To thus misread a hypothetical proposition is, therefore, to conceive a relation as holding in reality which does not hold at all. Of course, the protasis may actually enunciate the *causa essendi*, and in many cases does so; that, indeed, is the character of the ideal judgment of science. But it is also true that in many cases it is not so, and the identity of form in the two cases opens the way to error.

176. Misinterpretation of Disjunctive Propositions.

Fallacy may arise from assuming a disjunctive proposition to contain exhaustive, co-ordinate, and mutually exclusive alternatives.

Fallacy may arise in dealing with a disjunctive judgment from assuming that it fulfils the conditions of ideal perfection, *i.e.*, that the alternative predicates are an exhaustive enumeration of co-ordinate and mutually exclusive species falling under the subject-genus. Many disjunctive propositions are far from fulfilling these conditions. Particularly must we avoid interpreting the alternatives as formally exclusive of each other (*cf.* § 79[1]). To argue that because all individual progress implies either moral or intellectual advance, therefore, if one advances morally one cannot advance intellectually, would be an example of fallacy traceable to this source. The characteristic fault of vicious dilemmas is, on the other hand, the undue assumption that the alternatives given exhaust all possible cases [*cf.* § 133 (iii.)[2]]. This is really at bottom a case of false opposition, and will be further considered in the next chapter. The misinterpretation of a disjunctive proposition which is to be used as the basis of a calculation of probability will obviously lead to the results being altogether inaccurate, and in this case the co-ordinate value of the alternatives is of equal importance with their exhaustiveness and exclusiveness (*cf.* § 157).

[1] First Edition, § 89. [2] First Edition, § 149 (iii.).

CHAPTER IV.

FALLACIES INCIDENT TO IMMEDIATE INFERENCE.

177. False Opposition.

Fallacy may be committed in each form of opposition

Any of the inferences based on the opposition of propositions discussed in § 97[1] may be wrongly performed, and we then have a fallacy of opposition. Thus, for example, to infer the falsity of the subaltern from that of its subalternans, or the truth of the subalternans from that of its subaltern would be such a fallacy. But the most dangerous and frequently committed fallacies of opposition are those connected with contradiction. The contrary may be confused with the contradictory. It is easy to state a number of alternatives and assume that they are all that are possible. As was noted in the last chapter, one form of misinterpretation of a disjunctive proposition is due to thus assuming that the alternatives given exhaust the possibilities. Under this head may be brought the last of Aristotle's fallacies *in dictione*, viz., that of Many Questions.

Plures Interrogationes.

Plures interrogationes = attempt to obtain a single answer to several questions.

The *Fallacia Plurium Interrogationum* or *Fallacy of Many Questions*, is the attempt to get a single answer to several questions asked in one, as in the old example : 'Have you left off beating your father ?' Other examples would be 'Where did you hide the goods you stole last night ?' 'Have you cast your horns ?' This last is a traditional example, and from it the fallacy is sometimes called the *Cornutus*. The whole is to-day quite frivolous, and the only justification for noting it as a separate class of sophism was that the com-

[1] First Edition, § 109

mon method of disputation amongst the Greeks was a procedure by question and answer. The essential nature of the fallacy is false opposition, due to the wrong application of the principle of Excluded Middle. The usual examples take the form 'Is it *x* or *y*?' and assume that it must be either *x* or *y*; that there is no other alternative. Thus the first example omits the alternative 'You never did beat your father,' and assumes that the only possible alternatives are 'You still beat your father' and 'You used to beat him but do so no longer.'

178. Illicit Conversion.

(i.) Abstract.

Illicit conversion is possible with **A** and **O** propositions.

To so convert a proposition that a term is used universally in the converse which was only used particularly in the convertend is a fallacy not infrequently committed. This is only possible with **A** and **O** categorical propositions, and with the hypothetical forms which correspond to them. When the fallacy is committed openly it may be called *abstract;* when it is hidden by the language it may be said to be *concrete.* The former have been already discussed with sufficient fulness [*see* §§ 102 (ii.), 105[1]]. Under the latter head come two of Aristotle's *fallaciæ extra dictionem*, viz., *Accidens*, where the illicit conversion is that of an **A** categorical proposition, and *Consequens*, where it is of a universal hypothetical.

(ii.) Accidens.

The fallacy of *accident* involves simple conversion of **A**.

The *Fallacia Accidentis* arises when a predication which can be correctly made of any subject is made of all the 'accidents' of that subject. But by 'accident' is here meant, not what is denoted by that name in Porphyry's Scheme of Predicables (*see* §§ 34, 38[2]), but, any subordinate part of a general notion. Thus, every species and individual is an accident of its genus, in this sense of the term. The fundamental invalidity in such inferences is the simple conversion of an **A** proposition, whence the correct conversion of that form of proposition is said to be *per accidens* [*see*

[1] First Edition, §§ 115 (ii.), 119. [2] First Edition, §§ 41, 45.

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§ 102 (ii.) (*a*)[1]. Thus, to take an example given by Aristotle: "Every triangle has its three angles equal to two right "angles; every triangle is a figure; therefore, every figure "has its three angles equal to two right angles." Here we have a spurious syllogism in *Darapti* with illicit process of the minor term. But if the minor premise were simply converted the formal argument would be a correct syllogism in *Barbara*, though with a false minor premise. The error is obvious. "Yet," says Grote, "Aristotle intimates that a "scientific geometer of his day, in argument with an un-"scientific opponent, would admit the conclusion to be well "proved, not knowing how to point out where the fallacy lay: "he would, if asked, grant the premises necessary for con-"structing such a syllogism; and, even if not asked, would "suppose that he had already granted them, or that they "ought to be granted," which, as Grote remarks, "affords "us a curious insight into the intellectual grasp of the "scientific men contemporary with Aristotle" (*Aristotle*, p. 391). Under this head should come a form of fallacy which is classed by De Morgan under the head of *æquivocatio*. 'To call you an animal is to speak truth, to call you an ass is to call you an animal; therefore, to call you an ass is to speak truth.'

Many logicians have failed to understand the nature of this fallacy, and have regarded it as but another name for the *Fallacia a dicto simpliciter ad dictum secundum quid* (*cf.* De Morgan, *op. cit.*, pp. 250-1; Jevons, *Elem. Less. in Logic*, p. 176).

(iii.) Consequens.

The fallacy of *consequent* involves either illicit conversion or illicit inversion of a hypothetical proposition.

The *Fallacia consequentis* was intended by Aristotle to denote simply the formal error of inferring the truth of the antecedent from that of the consequent, or the falsity of the consequent from that of the antecedent [*cf.* § 131 (ii.)[2]]. In the former case the error involved is that of invalid conversion; in the latter case we have an instance of illicit inversion (*cf.* § 180). Like other fallacies, the invalidity is often hidden by the length and complexity of the argument in

[1] First Edition, § 115 (ii.) (*a*). [2] First Edition, § 147 (ii.).

which it occurs. The most dangerous form of it is, undoubtedly, the assumption that a conclusion is necessarily wrong because it is supported by invalid arguments, or, conversely, that the arguments urged in support of a proposition accepted as true must necessarily be cogent. In both these cases personal bias and prejudice have abundant scope to come into operation. When a conclusion is deduced from an invalid argument, we are only justified in saying that such a conclusion is not proven, not that it is disproved. In any case, it must be remembered, a proposition can only be disproved by a cogent argument establishing its contradictory.

179. Illicit Contraposition.

Illicit contraposition involves illicit conversion.

Contraposition is illicit when the conclusion contains a distributed term which was undistributed in the premise. As contraposition involves both obversion and conversion, it will be found that the invalidity is always in the latter process. Thus, a fallacy of contraposition is only possible when the given premise is an **E** or **I** proposition or a hypothetical proposition of corresponding form [*cf.* §§ 102 (iii.); 105[1]].

180. Illicit Inversion.

Illicit inversion draws an unjustified universal conclusion.

To infer from *Every S is P* that *No $\bar{S}$ is P* or from *No S is P* that *Every $\bar{S}$ is P* would be to commit the fallacy of illicit inversion [*cf.* §§ 102 (iv.); 105[2]]. Thus, to argue that 'Thought is existence, therefore, what contains no element of thought is non-existent' or that 'A bad man must be miserable because happiness is the result of well-doing' is to commit this fallacy. Similarly to assume that because the proposition *If S is M it is P* is established, therefore, we are justified in assuming that *If S is not M it is not P* would be to commit this fallacy. Inductive enquiry is somewhat liable to this error. The sufficiency of a positive condition to secure a given result must not, however, be held to demonstrate that the result could be attained in no other way.

[1] First Edition, §§ 115 (iii.), 119.

[2] First Edition, §§ 115 (iv.), 119.

Because a certain flower is fertilized when it is visited by insects, it does not follow that it could not be fertilized if no insects came near it. Whether it is so or not must be settled by a special enquiry conducted under conditions which exclude the positive agent already known to be sufficient to secure the result.

CHAPTER V.

FALLACIES INCIDENT TO DEDUCTIVE INFERENCE.

181. Abstract Fallacies.

Abstract syllogistic fallacies— 1. Undistributed Middle, 2. Illicit Major, 3. Illicit Minor—

Violation of any of the fundamental rules of syllogisms may be either open, or hidden by the ambiguities of language. In the former case we have abstract syllogistic fallacies, and in the latter case concrete syllogistic fallacies. The former we have already dealt with in connexion with our consideration of the rules of syllogism (*see* § 111[1].) They take one of the three forms—

(i.) Undistributed Middle.
(ii.) Illicit Process of the Major Term.
(iii.) Illicit Process of the Minor Term.

are all reducible to four terms.

It is sufficient here to add to what was there said that these are all at bottom instances of the most general syllogistic fallacy of *four terms*. To have an undistributed middle is formally equivalent to having two distinct middle terms, for there is no assurance that the reference is in both cases to the same part of the denotation of that term, and we cannot, therefore, assume in any case that it is so. Similarly, if illicit process, either of the major or of the minor term, is committed, we have again four terms in the apparent syllogism, for the extreme term which is illicitly distributed in the conclusion is different in its reference from the corresponding extreme term contained in the premises. In every case of syllogistic fallacy, therefore, the apparent syllogism is really a 'logical quadruped.'

[1] First Edition, § 125.

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182. Concrete Fallacies.

Ambiguous language leads to fallacies of four terms.

Not different in the ultimate form of the error are the concrete syllogistic fallacies, though in them the fallacy is veiled by the use of ambiguous language. But where these ambiguities have been pierced through, the fallacy in every case stands revealed as an instance of four terms. All the cases of fallacy due to ambiguities of language which we have already considered in chapters ii. and iii. of the present Book lead to such syllogistic fallacy when the propositions containing them are used in syllogistic argument. As we mentioned in § 111[1], the ambiguity is most frequently found in the middle term, though this is not always the case. The fallacy of composition or division, for instance, is generally based on a confusion between the collective and distributive use of the minor term. It is unnecessary to dwell further on this class of fallacies, as they have already been fully discussed.

[1] First Edition, § 125.

CHAPTER VI.

FALLACIES INCIDENT TO INDUCTIVE INFERENCE.

183. Imperfect Observation.

As all inductive inference starts from concrete phenomena as the basis of every hypothesis, and continually returns to concrete phenomena as the test of accuracy, it is evident that its validity depends directly upon the correctness and perfection of the observations of reality upon which it is based. But the discussion in Book V., ch. v., made it abundantly manifest that observation of given phenomena is a process of extreme difficulty and delicacy, and, consequently, one in which it is very easy to go wrong. The conditions which must be fulfilled by a perfect observation were set forth in § 153, and some of the most prominent sources of error were there mentioned. We will now, however, consider more fully and systematically the main kinds of error to which observation is liable. Error which is due to mere personal idiosyncrasy or to carelessness, we are, of course, not concerned with. Logic only considers those sources of error to which observers in general are subject.

Induction is based on observation, and observation is liable to error.

In the first place, then, as Mill points out, "a fallacy of "misobservation may be either negative or positive; either "Non-observation or Mal-observation. It is non-observation "when all the error consists in overlooking, or neglecting, "facts or particulars which ought to have been observed. "It is mal-observation when something is not simply un-"seen, but seen wrong; when the fact or phenomenon, "instead of being recognized for what it is in reality, is "mistaken for something else" (*Logic*, V., iv., § 1]. It will

Fallacies of observation are of two kinds—
1. Non-observation.
2. Mal-observation

be most convenient to consider these two classes of imperfect observation in succession.

(i.) Non-observation.

Non-observation is failure to take account of instances or conditions which affect the result.

It was pointed out in § 153 (i.) that all observation implies selection and isolation of phenomena. In making this selection it is evidently easy to overlook or to neglect either instances or conditions which are pertinent to the matter in hand. The former case would be most likely to happen in the earliest stage of inductive enquiry, when by simple enumeration of instances an attempt is made to determine exactly what is the character of the phenomena to be explained. The latter would be most easily committed at a later stage of the process, when an analysis of phenomena has been entered on for the purpose of testing and moulding a hypothesis.

Neglect of instances is frequently due to bias.

(*a*) *Neglect of Instances.* Probably the most fertile source of the error of passing over instances pertinent to the subject in hand is bias. We have a natural tendency to dwell upon instances which make for the theory we would like to see established, or which we have hitherto held, and to treat as of no importance those which make against it, or even to neglect to consider them at all. A striking example of this is mentioned by Mill: "The opponents of Copernicus "argued that the earth did not move, because if it did, a "stone let fall from the top of a high tower would not "reach the ground at the foot of the tower, but at a little "distance from it, in a contrary direction to the earth's "course; in the same manner (said they) as, if a ball is let "drop from the mast-head while the ship is in full sail, it "does not fall exactly at the foot of the mast, but nearer "to the stern of the vessel. The Copernicans would have "silenced these objectors at once if they had *tried* dropping "a ball from the mast-head, since they would have found "that it does fall exactly at the foot, as the theory requires: "but no; they admitted the spurious fact and struggled "vainly to make out a difference between the two cases" (*ibid.*, V., iv., § 3). This influence of bias is not surprising

when it is remembered how largely observation involves implicit inference and is guided by previous knowledge.

Positive instances are frequently observed, and negative ones neglected.

A very common form in which non-observation of instances appears is when attention is directed to positive instances, whilst negative instances are neglected. This originates in a kind of natural tendency of the mind to be impressed by any occurrence and to omit to notice non-occurrence. To this is due many superstitions which take the general form of attributing causal connexion to what is merely casual coincidence. For instance, the belief in the prophetic character of dreams is based upon taking note of the few cases in which a dream bears some resemblance to succeeding or simultaneous events, and passing over entirely those infinitely more numerous cases in which the dream is not 'fulfilled.' The theory of probability would prepare us to expect some such coincidences—especially as a small degree of resemblance is always liberally interpreted—and that without any necessary connexion. "In former generations," remarks Dr. Fowler, "'coincidences' of this kind were "regarded not simply as 'curious' and 'remarkable,' but "as proofs of some causal connexion between the events. "To talk of a person was supposed to render his presence "more likely; a verified prediction was regarded as evidence "of second-sight; and a comet which was observed to be "followed by a war was supposed to be, if not the cause of "the war, at least a messenger sent from Heaven to pro-"claim its approach" (*Ind. Log.*, p. 256).

Many instances of non-observation of facts are to be found in the history of science. Some remarkable ones have been already given in a passage quoted from Whewell in § 150 (i.) (*see* p. 85 *supra*).

The most extreme case of non-observation is the invalid inference that because a phenomenon has never been observed it is necessarily non-existent. This we have already dealt with [*see* § 153 (i.)].

(*b*) *Neglect of Operative Conditions.* In § 153 (iii.) it was shown that in every examination of concrete reality a residue of unanalysed elements remains, and that this opens a

possibility of leaving out of consideration some element which is an essential part of the phenomena we are investigating.

Neglect of operative conditions is specially guarded against by inductive methods.

The whole intention of all inductive methods is to eliminate those elements, and those elements only, which are not operative in respect to the aspect of reality under examination. In a science still so largely in the empirical stage as medicine, this fallacy is still not unknown and was formerly extremely common. Take as an instance Kenelm Digby's sympathetic powder which in the seventeenth century attracted considerable attention even from scientific men. "The sympathetic "powder was that which cured by anointing the weapon "with its salve instead of the wound. I have long been "convinced that it was efficacious. The directions were to "keep the wound clean and cool, and to take care of diet, "rubbing the salve on the knife or sword. If we remember "the dreadful notions upon drugs which prevailed, both as "to quantity and quality, we shall readily see that any way "of *not* dressing the wound would have been useful" (De Morgan, *Budget of Paradoxes*, p. 66). Those who attributed the cure to the powder committed the error of overlooking the essential circumstance that nature was left to her own devices under favourable conditions for effecting a cure.

This fallacy is specially common in inferences on social and economic subjects.

Owing to the complexity of the phenomena, all inductions connected with social and economic subjects are particularly liable to this form of fallacy. An increase in the number of convictions for any particular crime must not be taken as a necessary proof of an increase in that particular form of criminality; it may be due to greater vigilance on the part of the police. An excellent example, given by Mill, is the following: "Take, for instance, the common notion, "so plausible at the first glance, of the encouragement given "to industry by lavish expenditure. A, who spends his "whole income, and even his capital, in expensive living, "is supposed to give great employment to labour. B, who "lives on a small portion, and invests the remainder in the "funds, is thought to give little or no employment. For "everybody sees the gains which are made by A's trades-"men, servants, and others, while his money is spending.

"B's savings, on the contrary, pass into the hands of the "person whose stock he purchased, who with it pays a debt "he owed to some banker, who lends it again to some "merchant or manufacturer; and the capital being laid out "in hiring spinners and weavers, or carriers and the crews "of merchant vessels, not only gives immediate employ-"ment to at least as much industry as A employs during "the whole of his career, but, coming back with increase "by the sale of the goods which have been manufactured "or imported, forms a fund for the employment of the "same and perhaps a greater quantity of labour in per-"petuity. But the observer does not see, and therefore "does not consider what becomes of B's money; he does "see what is done with A's: he observes the amount of "industry which A's profusion feeds; he observes not the "far greater quantity which it prevents from being fed; "and thence the prejudice, universal to the time of Adam "Smith, that prodigality encourages industry, and parsimony "is a discouragement to it" (*op. cit.*, V., iv., § 4).

(ii.) Mal-Observation.

Mal-observation is mis-interpretation of sense impressions.

By mal-observation is meant the wrong interpretation of sense impressions. It has been pointed out several times in the course of the present work that all observation involves implicit inference, or the interpretation of what is received through the senses by referring it to some part of the knowledge previously possessed. When this reference is wrongly performed, we have a case of mal-observation. The rustic who takes a tombstone brightened by the rays of the moon for a ghost, or who interprets a donkey's bray as the voice of a departed ancestor, falls into the fallacy we are now considering. All conjurers' tricks appeal to the innate facility with which mankind observes badly. It is, however, never the senses that are wrong; the sensuous impression as received is just what it should be under the circumstances; in the meaning given to that impression the error will always be found to lurk. One of the most striking examples of mal-observation was the universal acceptance of

the Ptolemaic solar system for many centuries. As Mill remarks, "People fancied they *saw* the sun rise and set, the "stars revolve in circles round the pole. We now know that "they saw no such thing; what they really saw was a set of "appearances equally reconcilable with the theory they held "and with a totally different one" (*op. cit.*, V., iv., § 5).

All illusions are cases of mal-observation.

Sometimes we find ourselves practically unable to interpret our sense impressions rightly, and we are then said to labour under an illusion. "To give one or two . . . examples of "the kind of illusion which the senses practise on us or "rather which we practise on ourselves, by a misinterpreta-"tion of their evidence: the moon at its rising and setting "appears much larger than when high up in the sky. This "is, however, a mere erroneous judgment; for when we "come to measure its diameter, so far from finding our con-"clusion borne out by fact, we actually find it to measure "materially less. Here is eyesight opposed to eyesight, with "the advantage of deliberate measurement. In ventriloquism "we have the hearing at variance with all the other senses, "and especially with the sight, which is sometimes contra-"dicted by it in a very extraordinary and surprising manner, "as when the voice is made to seem to issue from an inani-"mate and motionless object. If we plunge our hands, one "into ice-cold water, and the other into water as hot as can "be borne, and after letting them stay awhile, suddenly "transfer them both to a vessel full of water at blood-heat, "the one will feel a sensation of heat, the other of cold. "And if we cross the two first fingers of one hand, and place "a pea in the fork between them, moving and rolling it "about on a table, we shall (especially if we close our eyes) "be fully persuaded we have two peas. If the nose be held "while we are eating cinnamon, we shall perceive no differ-"ence between its flavour and that of a deal shaving" (Herschel, *Nat. Phil.*, § 72).

Logical rules can neither guard against this fallacy nor detect it.

Dangerous as this fallacy is, Logic can neither give special rules for avoiding it, nor determine when it has been committed. The latter point is decided by further advance in that particular branch of knowledge. On the former the

only rule that can be given is the very general one that every observer should equip himself with as much definite knowledge of the subject under investigation and cognate subjects as he possibly can.

184. False Analogy.

False analogy will give rise to wrong hypothesis.

As hypotheses are suggested by analogy, it follows that if the analogy appealed to is a false one, the hypothesis will be wrong and the enquiry will be started on a false scent. This is continually happening in scientific enquiries; further examination of phenomena constantly leads to modification or rejection of the hypothesis [*cf.* § 150, (i.)]. In the former case the analogy on which the hypothesis was based was partly, and in the latter case wholly, erroneous.

When the force of the analogy is wrongly estimated, the hypothesis is partly wrong.

When an analogy leads to a hypothesis which is afterwards found to be only partially true, and, therefore, to need modification, it is because the force of the analogy has been wrongly estimated. The analogy is really present, and really suggests a hypothesis of this general character. But the points of identity have been allowed too much weight relatively to the points of difference with which they are bound up [*cf.* § 149 (iii.)], and, as a consequence, the hypothesis is found to break down in detailed application. Here, it will be seen, we have really a fallacy of non-observation of conditions as the foundation of our false analogy [*cf.* § 183 (i.), (*b*)].

One case of this is when the point of the analogy is missed.

One special case of false estimation of the force of an analogy is when the exact point of the analogy is missed. In this case, points of identity which are important relatively to one purpose are made the basis of an analogy directed to another purpose. The importance of points of identity we have already seen to be relative to purpose [*see* § 149 (ii.)]. To apply to one end an analogy which holds for another end may, therefore, yield a hypothesis which may be of any degree of inaccuracy—partial or total—according to the remoteness of the two purposes from each other. The argument, humorously put into the mouth of Socrates by Plato in the *Republic*, "that if justice consists in keeping property

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"safe, the just man must be a kind of thief; for the same "kind of skill which enables a man to defend property, will also "enable him to steal it" (*Mackenzie, Manual of Ethics*, p. 19), is an instance of this species of false analogy. The identity in skill is not important in reference to the question under consideration. For "justice is not a kind of skill, but a "kind of activity. The just man is not merely one who "*can*, but one who *does*, keep property safe. Now, though "the *capacity* of preserving property may be identical with "the *capacity* of appropriating it, the *act* of preserving "is certainly very different from the act of appropriating" (*ibid.*).

To assume analogy where none exists leads to entirely wrong hypothesis.

To mistake the exact bearing of an analogy may, then, lead to as much error as to assume the existence of an analogy where none exists; in fact, utter mistake of the bearing of an analogy will probably give rise to the application of it to cases on which it has no real bearing. The use of metaphorical language is a frequent source of such errors; in those cases the false analogy may be said to be based on, and to grow out of, a fallacy of Figure of Speech [*cf.* § 171 (iii.)]. Thus, for instance, the speaking of a 'mother-country' in relation to colonies may lead to the inference that the relations of a colony to the 'mother-country' are identical with those of children to their mother, and hence to inferences as to the duty of colonists to the 'mother-country' of a very erroneous character. For the points of identity in the two relations are not fundamentally important in this connexion, whilst the points of difference are. The relations of natural affection, early dependence, and constant association, which are operative to the fullest extent in the family, are operative, either not at all or only to a very small extent, in the relation between the old country and its colonies; and moreover, the right of the colony to pursue its own self-development may cause a conflict of interests with which there is no analogy in the case of the family.

Metaphorical language frequently gives rise to false analogy.

Examples— 1. 'Mother-country' and colonies.

2. Growth of metropolis.

A similar instance of false analogy due to metaphorical language is when the growth of the metropolis is condemned because it is called the 'heart' or the 'head' of the body.

As abnormal increase of these members points to disease in the natural body it is held that analogous diseases will follow in the body politic if the capital of the country becomes very large. Thus Smollett in *Humphry Clinker* says of London: "The capital is become an overgrown monster; which, like "a dropsical head, will in time leave the body and extremi- "ties without nourishment and support. . . . What wonder "that our villages are depopulated, and our farms in want of "day-labourers?" Now, the abnormal influx of labour into the great towns from the country is, undoubtedly, a social evil of considerable magnitude. But the growth of the towns is the effect of this influx, not its cause, and the consequent evils are not essentially analogous to "leaving the body "and extremities without nourishment and support."

3. Pilot and governor.

Arguments of this character were common amongst the Greeks, who attached undue importance to words and the analogies they suggest. That the governor of a state is like a pilot, for instance, is true in some points of not first-rate importance; but that his functions are unlike those of a pilot in many other points which are of great importance is also true.

4. Community and individual.

A very common false analogy is that between a community and an individual. As the latter goes through successive periods of growth, maturity, and decay, so the hypothesis is formed that every nation must do the same, and it is assumed that after a longer or shorter period of vigour and prosperity the nation must gradually lose its place in the world. The disastrous consequences of such a theory upon the energy of the nation in general and its leaders in particular are obvious and can scarcely be exaggerated. And history lends some colour to the theory in recounting the rise and fall of great empires. But examination of the circumstances tends to show that such decay is not a necessary consequence of the continued life of the state; it occurs at the end of periods more or less protracted, and may in every instance be explained by conditions independent of the mere duration of the community as an organized state. But in the case of the individual organism, senile decay is a direct result of the

constitution of the body, which is such that after a certain interval, decay exceeds recuperation and at last necessarily ends in death. The fall of great empires is more analogous with death from disease than with death from old age. Hence, there is no reason to regard such decay of power and prosperity as the necessary end of a lengthened existence. The falseness of the analogy is well shown in the following passage from Burke's *Letters on a Regicide Peace*: "I am not "quite of the mind of those speculators who seem assured "that necessarily, and by the constitution of things, all "states have the same periods of infancy, manhood, and "decrepitude, that are found in the individuals who compose "them. Parallels of this sort rather furnish similitudes to "illustrate or to adorn, than supply analogies from whence "to reason. The objects which are attempted to be forced "into an analogy are not found in the same classes of exist-"ence. Individuals are physical beings, subject to laws "universal and invariable. The immediate cause acting in "these laws may be obscure: the general results are subjects "of certain calculation. But commonwealths are not physical "but moral essences. They are artificial combinations, and, "in their proximate efficient cause, the arbitrary productions "of the human mind. We are not yet acquainted with the "laws which necessarily influence the stability of that kind "of work made by that kind of agent" (*Works*, vol. viii., pp. 78-9).

It is a fallacy to regard analogy as furnishing proof.

A common fallacy of analogy is to regard an analogy as a proof. But, as was shown in § 149, the conclusion of an argument from analogy can only be problematic, and the true function of such an argument is to suggest hypotheses and lines of enquiry.

185. Illicit Generalization.

All inductive fallacies have an element of illicit generalization.

As generalization is throughout involved in induction, all the fallacies discussed in this chapter have about them an element of illicit generalization. When this is made explicit, and a general statement is made for which there is not sufficient evidence, we have a fallacy which is primarily one of

Explicit fallacies of generalization are very common,

generalization. No fallacies are more common than this. The human mind is naturally disposed to generalize its experience in a very rash manner. This tendency is prominent in the case of children, and, on the other hand, caution in making general assertions is one of the distinctive marks of the exact thinker. But the majority of people are not exact thinkers, and hence, as De Morgan says: "In our day "nothing is more common than to hear and read assertions "made in all the form, and intended to have all the power, "of universals, of which nothing can be said except that most "of the cases are true. If a contradiction be asserted and "proved by an instance, the answer is 'Oh! that is an *extreme* "*case.*' But the assertion had been made of *all* cases. It "turns out that it was meant only for ordinary cases; why "it was not so stated must be referred to one of three "causes;—a mind which wants the habit of precision which "formal logic has a tendency to foster, a desire to give more "strength to a conclusion than honestly belongs to it, or a "fallacy intended to have its chance of reception. The "application of the *extreme case* is very often the only test by "which an ambiguous assumption can be dealt with. . . . "Where anything is asserted which is true with exceptions, "there is often great difficulty in forcing the assertor to "attempt to lay down a canon by which to distinguish the "rule from the exception. Everything depends upon it: "for the question will always be whether the example "belongs to the rule or the exception. When one case is "brought forward which is certainly an exception, the assertor "will, in nine cases out of ten, refuse to see why it is "brought forward. He will treat it as a fallacious argument "against the rule, instead of admitting that it is a good "reason why he should define the method of distinguishing "the exceptions: he will virtually, and perhaps absolutely, "demand that all which is certainly exception shall be kept "back, simply that he may be able to assume that there is no "occasion to acknowledge the difficulty of the uncertain "cases" (*Formal Logic*, pp. 270-1). As was pointed out in § 154 (i.), the very essence of the process of establishing a

and 'extreme cases' are frequently ignored.

universal proposition is to exactly define its limits by a careful examination of negative instances, and by so moulding its expression that it shall hold true without exception. No proposition against which a single exception can be brought is really universal, and to assert it in universal form is the fundamental essence of the fallacy of generalization. "A "rule may have exceptions, it is said; but this is hardly a "correct statement. A rule with exceptions is no rule, "unless the exceptions be definite and determinable: in "which case the exceptions are exclusions *by another rule*" (De Morgan, *ibid.*, p. 279).

Generalizations are of two degrees of strength and validity, and there are tendencies to error peculiar to each. We will briefly glance at these in order.

(i.) Empirical Generalizations.

It is illicit to regard an empirical generalization as a universal law.

The fundamental error in connexion with empirical generalization is to regard such a description of experience as a universal and necessary law. An empirical generalization can, at the best, never attain more than a high degree of probability; it can never be certain [*see* § 160 (ii.)]. One instance in which this tendency to elevate a statement of unexplained regularity of occurrence into a necessary law is operative is the assumption of the necessary character of the averages which are found to be so strikingly constant in many social phenomena. With this we have already dealt [*see* § 160 (ii.)]. In every case when the generalization is merely empirical, only a partial analysis of the given phenomena has been made. Thus this class of fallacy is very closely connected with that of imperfect observation (*cf.* § 183). An empirical generalization is usually based upon some striking feature common to the cases examined.

This fallacy is specially incident to enumerative induction.

Hence, it is emphatically the fallacy incident to induction by simple enumeration. Take an example quoted by Mill which is still, it may be suspected, by no means uncommon: "One "poor person in a thousand educated, while the nine hundred "and ninety-nine remain uneducated, has usually aimed at "raising himself out of his class, therefore education makes

"people dissatisfied with the condition of a labourer" (*Logic* V., v., § 4). Here the invalid generalization is obviously due to the neglect to notice that it is not education by itself but the exceptional education of one individual, whilst the many remain ignorant, which is the cause of the dissatisfaction noticed, and that, consequently, when education becomes general, it ceases to thus distinguish a labourer from his fellows, and the cause of the discontent, therefore, disappears.

Fallacy is committed when empirical generalizations are extended to different conditions.

An empirical generalization, we have seen, can only be extended with any confidence to closely analogous cases. Thus, to extend such a generalization founded upon a survey of the social condition of any one country at any particular time to other times and other peoples—especially if these latter are in a different stage of civilization—is to commit the fallacy of illicit empirical generalization. Owing to the complexity of social phenomena, most social generalizations are empirical; thus, in this domain of knowledge the liability to fall into this particular fallacy is specially great.

Undue respect for authority is a fallacy of this class.

As one class of fallacies of this character Dr. Fowler mentions undue respect for authority. The influence on the opinion of the multitude of expressed judgments of well-known people on certain questions is well known to advertisers of quack medicines, promoters of public companies, and those generally who wish to attract public confidence to the goods they have to sell. The form of the argument involved would seem to be, that A is a man whose opinion on many subjects is of weight, therefore, his judgment is of weight on all subjects. "We have to learn," says Dr. Fowler, "not only that men are to be trusted exclusively within the "limits of their own experience, in their own profession or "pursuit, but that even within those limits their authority "is apt to become tyrannical and irrational unless it is con-"stantly confronted with facts and subjected to the criticism "of others" (*Ind. Log.*, p. 292). Similar remarks apply to an excessive regard for the authority of great writers. A striking example of this fallacy is found in the intellectual idolatry with which the schoolmen regarded Aristotle.

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It is a fallacy to assume that invariable connexion proves causation.

One very common form of the fallacy we are now considering is that known by the scholastics as *Post* (or *Cum*) *hoc, ergo propter hoc.* Because two events have been frequently, or even invariably, conjoined in experience, either successively or simultaneously, to assume a causal connexion between them is a characteristic fallacy of enumerative induction. Such conjunction, of course, gives a probability that there are some constant essential conditions, and this probability increases with the scope of the experience [*see* § 157 (ii.) (*d*)]. But only a thorough analysis of the conditions gives us a right to assert causal connexion.

(ii.) Laws of Nature.

Illicit generalization of laws is due to inadequate analysis of conditions.

True laws are established when the exact conditions of a phenomenon are exhaustively and definitely known [*see* § 160 (i.)]. In the ascertainment of these conditions, as became apparent in our previous discussion, error may be committed either by not arriving at an accurate determination of conditions which are the ground of a phenomenon, or by not correctly determining to what cases of concrete reality the law is applicable [*see* § 160 (i.)]. Of this latter fallacy the most common examples are probably to be found in the consideration of cases where the operation of the law is frustrated by counteracting conditions. Especially is this liable to occur in the consideration of social questions. In illustration we will quote a passage from Sir G. C. Lewis: "It is to be borne in mind that, in estimating negative "instances, due allowance must be made for the occasional "*frustration of causes.* . . . For example : it might be argued, "from the occurrence of several cases in which the absence "of high import duties and of commercial restrictions was "accompanied with abundance and cheapness of commodi-"ties, that the former was the cause of the latter. Certain "instances might then occur, in which the former existed "without the latter ; but each of these exceptional cases "might be accounted for, by showing that there was a "special circumstance, such as a deficient supply, or inter-"ruption of intercourse by war or blockade, which partially

It is a fallacy to disregard counteracting causes.

"obstructed, and for a time suspended, the operation of the "former cause. Again: it might be shown, by the evidence "of facts, that the operation of a new law had been gene- "rally beneficial, with the exception of certain districts, "where its enforcement had been prevented or retarded by "certain peculiar and accidental circumstances. Exceptions "of this kind, which admit of an adequate special explana- "tion, serve rather to confirm the general inference than to "weaken it; inasmuch as they raise the presumption that, "but for the partial obstruction to the cause, it would have "operated in these as in the other instances where no ob- "structions existed" (*Methods of Observation and Reasoning in Politics*, vol. i., p. 386).

The fallacy may consist in omission, or wrongful inclusion, of elements.

In such cases we have, of course, an inadequate analysis of the conditions of the case to which we propose to apply the general law. And so, generally, all illicit statement of law, and all wrong application of law, resolve themselves into inadequate analysis of the Given. The statement of the law is erroneous when the conditions given include either more or less than just those essential conditions which are the ground of the phenomenon [*see* § 160 (i.)]. Thus, if we take one out of a number of related conditions as the total cause, our general law will be wrong, as its statement will embrace instances which do not really come under it. For example, to state that the boiling-point of a liquid depends only on the temperature would be to omit the equally essential condition of atmospheric pressure. Thus, to say that water boils at 100° C. is wrong; it boils at that temperature under the pressure of one atmosphere; *i.e.*, the normal atmospheric pressure at the sea-level. Up a mountain the boiling-point is different.

Fallacy may be due to confusion between cause and effect,

Again, in the analysis of phenomena it is possible to confuse the whole or a part of the cause with the effect. That it is by no means easy to determine in all cases which are the determining, and which the determined, elements in a complex phenomenon is illustrated by the fact that meteorologists are not agreed whether the copious and sudden downfalls of rain which usually attend thunder-storms are

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the cause or the effect of the electric discharge. The common opinion is that they are the effect, but Sir John Herschel held that they were the cause.

or to neglect of the mutual interaction of cause and effect.

Another source of error is the neglect of the mutual interaction of cause and effect. This interaction, of course, may also lead to that confusion of cause and effect which we have just noticed. This again is well illustrated from social phenomena by a passage from Sir G. C. Lewis. He says: "An additional source of error in determining political "causation is likewise to be found in the *mutuality of cause* "*and effect.* It happens sometimes that, when a relation of "causation is established between two facts, it is hard to "decide which, in the given case, is the cause and which the "effect, because they act and re-act upon each other, each "phenomenon being in turn cause and effect. Thus, habits "of industry may produce wealth, whilst the acquisition of "wealth may promote industry; again, habits of study may "sharpen the understanding, and the increased acuteness of "the understanding may afterwards increase the appetite for "study. So our excess of population may, by impoverishing "the labouring classes, be the cause of their living in bad "dwellings; and again, bad dwellings, by deteriorating the "moral habits of the poor, may stimulate population. The "general intelligence and good sense of the people may pro-"mote its good government, and the goodness of the govern-"ment may, in its turn, increase the intelligence of the "people, and contribute to the formation of sound opinions "among them. Drunkenness is in general the consequence "of a low degree of intelligence, as may be observed both "among savages and in civilized countries. But, in return, "a habit of drunkenness prevents the cultivation of the "intellect, and strengthens the cause out of which it grows. "As Plato remarks, education improves nature, and nature "facilitates education. National character, again, is both "effect and cause: it reacts on the circumstances from which "it arises. The national peculiarities of a people, its race, "physical structure, climate, territory, etc., form originally "a certain character, which tends to create certain institu-

"tions, political and domestic, in harmony with that character. "These institutions strengthen, perpetuate, and reproduce the "character out of which they grew, and so on in succession, "each new effect becoming, in its turn, a new cause. Thus "a brave, energetic, restless nation, exposed to attack from "neighbours, organizes military institutions: these institutions "promote and maintain a warlike spirit: this warlike spirit "again assists the development of the military organization, "and it is further promoted by territorial conquests and "success in war, which may be its result—each successive "effect thus adding to the cause out of which it sprung" (*op. cit.*, vol. i., p. 375).

CHAPTER VII.

FALLACIES INCIDENT TO METHOD.

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186. Undue Assumption of Axioms.

Proof must start from axioms or self-evident propositions.

All proofs must ultimately start from certain propositions received as true without proof, as otherwise we should be landed in an infinite regress in our search for proof. It follows, therefore, that some propositions are of so self-evident a character that they must be accepted as true by every rational mind immediately the terms in which they are expressed are understood. Such propositions alone should be received as axioms. The laws of thought and the fundamental assumptions of mathematics are examples of true axioms.

False assumption of axioms is due to confusion as to the relation of thought and matter.

The determination of what are the fundamental axioms of all knowledge would take us beyond the limits of our subject into the domain of metaphysics. But it may be pointed out that the origin of such errors as the unjustified assumption of axioms is to be sought in confused ideas of the relation of thought to matter. As we have abundantly seen in our discussion of induction, thought and matter are not separate and independent realms of existence, but only two aspects of reality. We understand reality just to the extent to which we think the elements of the world are related in the way in which they are related (*cf.* § 141). The sole test of truth is the relation of any one judgment to the whole system of knowledge, and the establishment of any judgment about reality, therefore, involves a constant appeal to experience [*cf.* § 160 (iii.)]. Now any separation of thought and fact is likely to lead to a wrong

assumption of axioms. On the one hand we may have the assumption of the *à priori* philosopher that the world can be deductively constructed in the study, by a development of certain concepts; and on the other, that the construction can be attained by a passive reception of sense impressions.

As instances of the former we may notice the tendencies of the Greeks to determine the nature of things from an examination of their names and the ideas attached to those names, involving the assumption that differences of language necessarily indicate difference of nature, and that affinities of language involved affinities of nature. This assumption was accepted without question as axiomatic.

An instance of the latter is the assumption that the relations between things must correspond to the connexions of our ideas of them. This underlies the error of deducing the doctrine of causation from the psychological effect of association. When two events have been invariably associated in our experience the idea of the one brings into consciousness the idea of the other; but it is unjustifiable to assume that the events are, therefore, causally connected. This spurious axiom may be said to underlie the whole doctrine of proof by enumerative induction.

187. Petitio Principii.

Principium with Aristotle meant axiom, but *petitio principii* now means assumption of the conclusion.

By a *principium* Aristotle means a truth which can be known of itself. *Petitio principii* is, therefore, committed when a proposition which requires proof is assumed without proof. In other words, in this sense, the *petitio principii* is the fallacy of undue assumption of axioms which we have just considered. By logicians generally the term is, however, used more widely to denote *petitio quæsiti*, or the assumption in some form of the very proposition to be proved, as a premise from which to deduce it. This may be done, Aristotle tells us, in five different ways :—by assuming the very proposition to be proved; by assuming, when the conclusion is particular, a universal which involves it; by assuming, when the conclusion is universal, a particular involved in it; by assuming piece by piece the proposition to

This may be made by assuming—
1. The same proposition;
2. A universal involving it;
3. A particular involved in it;

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be proved; by assuming a proposition which necessarily implicates the proposition to be proved. The assumption, it will be seen, is in every case of the same character. The two first modes of committing the fallacy are, however, of the greatest importance and of the most frequent occurrence.

4. The proposition part by part; 5. A proposition implying it.

Direct assumption of the conclusion is made possible by use of synonyms.

The direct assumption of the proposition to be proved would seem to be hardly possible when it is expressed on both occasions in the same terms. Still some people can be found who commit it. Commenting on an attempt to square the circle published by a Mr. James Smith in a work entitled *Nut to Crack*, De Morgan says: "Mr. Smith's method of "proving that every circle is $3\frac{1}{8}$ diameters is to assume that "it is so,—'if you dislike the term datum, then, by hypothesis, "let 8 circumferences of a circle be exactly equal to 25 "diameters,'—and then to show that every other supposition "is thereby made absurd. The right to this assumption is "enforced in the 'Nut' by the following analogy:—'I think "you (!) will not dare (!) to dispute my right to this hypo-"thesis, when I can prove by means of it that every other "value of π will lead to the grossest absurdities; unless, "indeed, you are prepared to dispute the right of Euclid to "adopt a false line hypothetically for the purpose of a "*reductio ad absurdum* demonstration in pure geometry.' "Euclid assumes what he wants to *disprove*, and shows that "his *assumption* leads to absurdity, and so *upsets itself*. Mr. "Smith assumes what he wants to prove, and shows that "*his* assumption makes *other propositions* lead to absurdity. "This is enough for all who can reason" (*Budget of Paradoxes*, p. 327). Such direct assumption is, however, by no means unusual when synonyms are used. Put symbolically, it being agreed that S is identical with A, and P with B, the fallacy is committed when the proposition A *is* B is assumed as a premise from which to prove S *is* P. This is, in the strictest sense, "begging the question." According to the directness of the invalid process two sub-forms may be distinguished—the *hysteron proteron* and the *circulus in demonstrando*.

It is *hysteron proteron* when only one step of inference is involved.

The *hysteron proteron* is the case when the fallacy is committed in a single step of inference, as 'The volume of a body diminishes when it is cooled, because the molecules

then become closer.' 'Opium induces sleep because it has a soporific quality.' It is obvious that the richer a language is in synonyms the more likely is a *hysteron proteron* to appear in arguments expressed in that language. Often a proposition expressed in abstract terms is given in proof of the same proposition expressed in concrete terms, as when we are told that the loadstone attracts iron because of its magnetic power, or that oxygen combines with hydrogen because it has an affinity for it. A good instance of this form of fallacy was unconsciously supplied by the member of Parliament who argued that 'the bill before the House is well calculated to elevate the character of education in the country, for the general standard of instruction in all the schools will be raised by it.'

The fallacy may consist in the use of 'question-begging epithets.'

The fallacy may even be committed in single words, by the use of what Bentham terms "question-begging appellatives," *i.e.*, those which beg the question under the pretence of stating it. "These," says Dr. Davis, "are potent when "laudatory, but even more so when vituperative; as "Radicals, Rebels, and most political catch-words. The "word 'innovation' having acquired a bad sense, the "admission, which is unavoidable, that a new measure is an "innovation is always construed to its disadvantage" (*Theory of Thought*, p. 284).

Even a trained mind may beg the question.

It must not be supposed that it is difficult to fall into this fallacy unintentionally, or that only persons of untrained minds can commit it. Galileo accuses Aristotle himself of having committed it in the following argument:—'The nature of heavy things is to tend towards the centre of the universe, and of light things to fly from it; experience proves that heavy things tend towards the centre of the earth and that light things fly from it; therefore, the centre of the earth is the centre of the universe.' But Aristotle could only say that heavy things tend towards the centre of the universe by assuming that centre to be identical with the centre of the earth, which is the very proposition he undertakes to prove (*cf. Port Royal Log.*, p. 249).

Put symbolically, the *hysteron proteron* takes either the form S *is* P, S *is* P. $\therefore$ S *is* P; or S is P, S is S, $\therefore$ S is P. An

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examination of the latter shows that whenever a definition is used as a premise in an argument meant to be demonstrative, the question is begged; and this Aristotle distinctly points out. For if we argue—'Every rectilinear three-sided figure has its angles equal to two right angles, Every triangle is a rectilinear three-sided figure, Therefore, every triangle has its angles equal to two right angles,' there is no proof. The major premise assumes the very point to be proved, for the minor premise does nothing but tell us how such a figure is named. Of course, such a use of a definition is permissible in explanations, though the form may be the same as the above; for when no demonstration is offered, no begging of the question is possible.

Reasoning in a circle involves more than one step of inference.

The *circulus in demonstrando* or *reasoning in a circle* is when the assumption of the conclusion as a premise is separated by a greater or less interval from its statement as a conclusion. It may be symbolically expressed thus:—*M is P*, *S is M*, ∴ *S is P*; *S is P*, *M is S* ∴ *M is P*. Of this Whately gives a good example: "Some mechanicians attempt to prove, (what they ought to "lay down as a probable but doubtful hypothesis), that every "particle of matter gravitates equally; 'Why?' 'Because "those bodies which contain more particles ever gravitate "more strongly, *i.e.*, are heavier: 'But (it may be urged) "those which are heaviest are not always more bulky'; 'No, "but still they contain more particles, though more closely "condensed'; 'How do you know that?' 'Because they are "heavier'; 'How does that prove it?' 'Because all particles "of matter gravitating equally, that mass which is specifically "the heavier must needs have the more of them in the same "space'" (*Logic*, p. 225). In illustration of this fallacy Hamilton says: "Plato, in his *Phædo* (p. 78), demonstrates "the immortality of the soul from its simplicity; and, in "the *Republic* (Bk. x., p. 611), he demonstrates its simplicity "from its immortality" (*Lectures on Logic*, vol. ii., p. 55).

When a universal which involves the conclusion is assumed there is no formal syllogistic fallacy.

The second mode of committing *petitio principii* is when a universal which involves the particular proposition to be proved is assumed. Here, there is no formal syllogistic fallacy. The argument takes the form of a valid syllogism

in *Barbara.* But it is an offence against the principles of Method, as the proposition assumed is one which needs proof quite as much as that which is inferred from it. This is the most accurate sense in which the term *petitio principii* is applied. A *principium*, as was said above, was a self-evident truth ; and the true *petitio principii* was the assumption of some proposition of inferior rank as such a principle. This was the sense in which the term was used by the scholastics. As De Morgan points out: "The "philosophy of the time consisted in a large variety of "general propositions (principles) deduced from authority, "and supposed to be ultimately derived from intrinsic "evidence, self-known, or else by logical derivation from "such principles. These were at the command of the dis-"putant, his opponent could not but admit each and all of "them : the laws of disputation received the assent which the "geometer requires for his postulates. Except when, now "and then, literary society was shaken to its foundations "by a dispute which affected any of them, as a nominalist "controversy, or the like moral earthquake. The most "frequent syllogism was one which, having the form "*Barbara*, had a *principium* for its major and an *exemplum* "for its minor: as in 'All men are mortal (*principium*); "Socrates is a man (*exemplum*); therefore, Socrates is a "mortal.' The *petitio principii*, then, occurred when any-"one, to prove his case, made it an example of a principle "which was not among those received, without offering to "bring the former under the logical empire of the latter" (*Form. Log.*, pp. 256-7). Of course, such a body of fixed and axiomatic principles, incapable of being added to, is not the basis of modern philosophy. But still, as was said above (*see* § 186), some truths must be accepted as not requiring proof, or all proof would be impossible, as there would be no starting point for the process. *Petitio principii* is, then, not committed whenever a proposition is assumed without proof, but when the proposition thus assumed is one which needs just the same kind of proof, bearing on just the same point, as the proposition which is to be

With the schoolmen this meant the undue assumption of axioms.

deduced from it. "Sound probation," says Dr. Davis, "must depart from such principles as are either immediately "given as ultimate, or mediately admit of proof from other "sources than the proposition itself in question" (*op. cit.*, p. 288).

Examples.

As examples of this form of the fallacy we may give: 'His cowardice may be inferred from his cruelty, for all cruel men are cowards'; 'A table of logarithms must be entertaining, for all books are so.' A striking example is found in the First Chapter of Mr. Spencer's *Education*. After stating that "acquirement of every kind has two "values—value as *knowledge* and value as *discipline*," Mr. Spencer goes on to discuss the value of different subjects from the point of view of knowledge. He then turns to the disciplinary value of studies, and commences his disquisition with the following flagrant *petitio*:—"Having "found what is best for the one end, we have by implica-"tion found what is best for the other. We may be quite "sure that the acquirement of those classes of facts which "are most useful for regulating conduct, involves a mental "exercise best fitted for strengthening the faculties. It "would be utterly contrary to the beautiful economy of "Nature, if one kind of culture were needed for the gain-"ing of information and another kind were needed as a "mental gymnastic."

It is this mode of the fallacy which is referred to when it is said that the syllogism is a *petitio principii* (*see* § 139[1]).

To assume the particular to prove the universal is of the nature of illicit enumerative generalization.

The other modes of *petitio principii* are not of much importance. The third mode—of assuming the particular to prove the universal which involves it—is of the nature of a generalization from simple enumeration. "Aristotle him-"self seems to be guilty of this when he maintains that "slavery is in accord with natural law, on the ground that "the neighbouring barbarians, being inferior in intellect, "are the born bondsmen of the Greeks" (Davis, *op. cit.*, p. 289. *See* Aristotle, *Politics*, i. 2).

To assume the conclusion piece by piece is equivalent to assuming it as a whole.

The fourth mode is only a variety of the first. Thus, to take Aristotle's example, when in trying to show that the healing art is knowledge of what is wholesome and un-

[1] First Edition, § 156.

wholesome, it is successively assumed to be the knowledge of each.

The question may be begged by assuming a reciprocal proposition.

The fifth mode is when a proposition which is in reciprocal relation to another proposition is assumed as a means of proving the latter. Aristotle's example is the assumption that the side of a square is incommensurable with the diagonal when the proposition to be proved is that the diagonal is incommensurable with the side. Other examples are: London is north of Brighton, therefore, Brighton is south of London; Philip was the father of Alexander, therefore, Alexander was the son of Philip; 'Everywhere the light of life and truth was lacking, for darkness covered the earth, and gross darkness the people.' In all these cases we have obviously no passage of thought; it is the same judgment which is expressed in different words; nor does the new expression unfold any meaning which was previously implicit.

188. Ignoratio Elenchi.

Ignoratio elenchi = proving the wrong conclusion.

By an *elenchus*—*i.e.*, a refutation—Aristotle meant a syllogism with a conclusion contradictory of the thesis to be refuted. The *ignoratio elenchi* was then applicable only to disputation, and consisted in arguing beside the mark, in answering to the wrong point, in establishing a proposition which did not overthrow the original thesis. But the scope of the fallacy may well be extended—as it usually is by modern logicians—to include all cases in which instead of the required conclusion, a proposition which may be mistaken for it is established. This might appropriately be called *ignoratio* or *mutatio conclusionis*. In every case the error consists in proving the wrong point. It is thus a violation of the fifth general rule of method (*see* § 164). As an example of *ignoratio elenchi* we may take the common argument against a classical education that "throughout his "after career, a boy, in nine cases out of ten, applies his "Latin and Greek to no practical purposes" (Spencer, *Education*, Ch. I.). This is to ignore the fact that the advocates of a classical education do not claim that Latin

and Greek are of direct use in practical life. What they do urge is that the study of the classics furnishes an unrivalled mental training; and it is this proposition which a true *elenchus* must disprove.

This fallacy is very common.

No fallacy is more common or more easily fallen into than *ignoratio*. Anyone who has had experience of disputations and debates knows how constantly recurrent is the tendency to wander from the real point at issue, especially when the subject under discussion is a wide one, and how necessary it is for a speaker occasionally to begin his remarks by reminding the disputants what the question under discussion really is. This tendency must be guarded against especially when any practically important results flow from the conclusion reached. Thus, as De Morgan tells us: "The *pleadings* in our courts of law, previous to "trial, are intended to produce, out of the varieties of "statement made by parties, the real points at issue, so that "the defence may not be *ignoratio elenchi*, nor the case the "counter fallacy . . . *ignoratio conclusionis*. If a man were "to sue another for debt, for goods sold and delivered, and "if the defendant were to reply that he had paid for the "goods furnished, and plaintiff were to rejoin that he could "find no record of that payment in his books, the fallacy "would be palpably committed. The rejoinder, supposed "true, shows that either defendant has not paid, or plaintiff "keeps negligent accounts; and is a dilemma, one horn of "which only contradicts the defence. It is plaintiff's business "to prove the sale from what is in his books, not the "absence of payment from what *is not*, and it is then "defendant's business to prove the payment by his vouchers" (*op. cit.*, p. 260).

The fallacy is committed when the burden of proof is thrown on the wrong side.

This leads on to that form of the fallacy which consists in throwing the burden of proof on the wrong side. Proof of an assertion should generally be given by the person who makes that assertion, and to endeavour to transfer to an opponent the task of proving the negative of that assertion is an *ignoratio elenchi*. It is often said that it is difficult, if not impossible, to prove a negative. And this is true so long

as the negative is a bare denial. But the establishment of every positive proposition proves a number of negatives. If, then, the number of possible alternatives are few, the proof of any one of them negates all the others. This principle is adopted in law. "For instance, a homicide, as such, "is considered by the law as a murderer unless, failing justi-"fication, he can prove he had no malice. . . . The case "stands thus:—the alternatives are few, so that proving the "negative of one, which the accused is called on to do, can "be done by proving the affirmative one out of a small "number. There is but malice, heat of blood, misadventure, "insanity, etc., to which the action can be referred. Of these "few things it is easier for the accused to establish some one "out of several, above all when motive is in question (of "which only himself can be in possession of the most perfect "knowledge), than it is for the prosecutor to establish a "particular one. Another principle on which he is called on "to establish a negative (or rather another positive) is that "the burden of proof fairly lies on the one to whom it will "be by much the easiest" (De Morgan, *ibid.*, p. 261).

To regard objections as necessarily fatal is a form of the fallacy.

One form of the fallacy is to confuse objections against the thesis proposed with its disproof. Especially is this likely to be committed when the question at issue is some proposed change, say in the law. Against most reforms some objections can be urged, but to treat these as necessarily fatal is an *ignoratio elenchi;* the point to be established is that those objections outweigh the reasons for the proposed change, and simply to point out their existence is entirely beside the mark. One common specimen of this form of the fallacy is to object to a certain conclusion as tending to establish a position deemed undesirable. Here we have what De Morgan calls "the great fallacy of all, the determination "to have a particular conclusion, and to find arguments for "it" (*ibid.*, p. 264), coming into play. The conclusion being fore-ordained, all arguments which make against it are refused a hearing.

Another form of the fallacy is to prove, or disprove, part of what is required, and to dwell on that to the exclusion of

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To treat part of the conclusion as the whole is to commit the fallacy.

the remainder. Thus, if one disputant supports a conclusion by weak arguments, his opponent may confine himself to showing the weakness of his arguments, and leave his readers or hearers to infer that, consequently, the conclusion drawn from those arguments is false. The handle thus given to the enemy should be a warning against the practice of urging bad arguments in support of a good cause.

An extreme form of this is to confuse an illustration with the conclusion.

A more extreme case of the same mode of committing the fallacy is the taking exception to a mere illustration or part of an illustration, which has no essential connexion with the point in dispute. The very use of illustration at all is liable to be an *ignoratio elenchi*. For an illustration is intended to make some point of difficulty clearer and easier of comprehension to the hearer or reader. But the user of the illustration may mistake the point which will need elucidation, and may illustrate the wrong point. And there may equally be an *ignoratio* on the part of the pupil. As De Morgan says: "The greatest difficulty in the way of learners is not knowing exactly in what their difficulty consists; and they are apt to think that when *something* is made clear, it must be *the* something" (*ibid.*, p. 266). And he rightly goes on to point out the danger incident to the use of concrete examples in the study of the rules of formal inference. "If the student receives help from an example stated both in matter and form, the odds are that the help is derived from the plainness of the matter, and from his conviction of the matter of the conclusion. . . . The right perception may, no doubt, be acquired by reflection on instances; but the minds which are best satisfied by material instances are also those which give themselves no further trouble" (*ibid.*, pp. 266-7). The use of illustrations is also liable to the fallacy in another way—the person to whom it is addressed may not see the analogy of the matter.

To substitute abuse for argument is to commit the fallacy.

One common and most objectionable form of *ignoratio elenchi* is summed up in the advice to counsel: 'No case; abuse the plaintiff's attorney.' Discussions, especially on subjects of real practical importance, such as politics and religion, are not usually conducted entirely in cold blood.

Prejudice and even bad faith lead members of one party to attribute all kinds of base motives to their opponents, and to discount all the statements they make. "The testimony "is, in the receiver's mind, of a low order; the proposer is a "radical, and the receiver is of opinion that a radical would "pick a pocket: or else, perhaps, the proposer is a tory, and "the receiver is of the belief that a tory must have picked a "pocket" (De Morgan, *ibid.*, p. 263). But it must be remembered that abuse is not argument, and that to prove any amount of ill conduct against the proposers of a certain measure or the maintainers of a certain proposition will not prove the measure unwise or the proposition untrue.

Other forms of *ignoratio elenchi* are—*Argumentum ad hominem*;

Closely allied to this form of the fallacy is the *argumentum ad hominem*, or the *tu quoque*—you're another—style of argument. All recrimination, all charges of inconsistency, are of this character, unless indeed the very point in dispute is personal character or consistency. But when a statesman brings forward a certain measure it is no objection to that measure to point out that he has hitherto opposed it. Very often, indeed, when a disputant tries to turn his opponent's former arguments against himself, it is not, as De Morgan says, "absolutely the same argument which is turned against "the proposer but one which is asserted to be like it, or "parallel to it. But *parallel cases* are dangerous things, "liable to be parallel in immaterial points, and diverge in "material ones" (*ibid.*, p. 265).

Argumentum ad baculum;

A sub-form of this species of the fallacy is the *argumentum ad baculum*. As Mr. Stock remarks: "To knock a man "down when he differs from you in opinion may prove your "strength, but hardly your logic" (*Deductive Logic*, p. 313).

Argumentum ad populum

Another form of *ignoratio* is the *argumentum ad populum*, or appeal to popular passion or prejudice. The following 'argument' against a literary education appears to us to be a striking instance: "When a mother is mourning over a "first-born that has sunk under the sequelæ of scarlet-fever "—when perhaps a candid medical man has confirmed her "suspicion that her child would have recovered had not its "system been enfeebled by over-study—when she is pros-

"trate under the pangs of combined grief and remorse; it is "but small consolation that she can read Dante in the "original" (Spencer, *Education*, Ch. I.).

Argumentum ad ignorantiam;

Closely allied to this is the *argumentum ad ignorantiam*, which consists in trusting that the ignorance of the hearer will lead to the acceptance as proved of statements which are by no means proved. And this is frequently allied with the *argumentum ad verecundiam* or appeal to a respected authority. 'You should accept this conclusion, or advocate this measure, because so-and-so supports it' is an *ignoratio elenchi* not infrequently heard in political life. This, as we have already seen, also involves a fallacy of illicit generalization [*see* § 185 (i.)]. Undue respect for authority leading to a neglect to examine the evidence for and against a given proposition is not a state of mind favourable to the detection of fallacy.

Argumentum ad verecundiam.

(ii.) Non Causa pro Causa.

Non causa = false reason.

The nature of this fallacy has been very frequently misunderstood through the ambiguity of the word *causa*. In modern phraseology the word 'cause' is almost always restricted to the *causa essendi*. But in the title of this fallacy Aristotle and his scholastic followers restricted it to the *causa cognoscendi* [*see* § 144 (i.)]. The fallacy is, therefore, not an inductive one; in other words it is not concerned with the determination of fact but with the justification of a judgment. "We mistake," says Aristotle, "for a cause what is not a "cause [*i.e.* for a reason what is not a reason] when an "irrelevant proposition has been foisted into an argument "as if it were one of the necessary premises" (*De Soph.*, V.). In the *Analytica Priora*, Aristotle termed the same fallacy *Non per hoc*, or *Non propter hoc.* Its essence consists in "the "pretence that the proposition we wish to refute is the "cause, in a *reductio ad impossibile*, of the false conclusion "which in fact flows from other premises; that is, the "sophism consists in maintaining that the conclusion is false "because *that particular premise* is false" (Davis, *Theory of Thought*, p. 290).

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It involves foisting an unnecessary thesis into a proof *per impossibile*.

The nature of the fallacy is clearly explained by Grote. He says: "When you intend to refute a given thesis by "showing, that, if admitted, it leads to impossible or absurd "conclusions, you must enunciate that thesis itself among "the premises that lead to such absurdities. But, though "enunciated in this place, it may often happen that the "thesis may be an unnecessary adjunct—not among the "premises really pertinent and essential: and that the im-"possible conclusion may be sufficiently proved, even though "the thesis were omitted. Still, since the thesis is declared "along with the rest, it will appear falsely to be a part of the "real proof. It will often appear so even to yourself the "questioner; you not detecting the fallacy. Under such "circumstances the respondent meets you by *Non propter hoc.* "He admits your conclusion to be impossible, and at the "same time to be duly proved, but he shows you that it is "proved by evidence independent of his thesis, and not by "reason or means of his thesis. Accordingly you have "advanced a syllogism good in itself, but not good for the "purpose which you aimed at; viz., to refute the thesis by "establishing that it led to impossible consequences" (*Aristotle*, p. 389). The fallacy can be detected by trying whether the premise in question can be omitted without interrupting the sequence of the argument. If it can, the fallacy has been committed. Of course, the superfluous premise may have been tacitly assumed, as in the example Aristotle himself gives: 'We assume that the opposite of destruction is generation; therefore, the opposite of a particular destruction is a particular generation; but death is a particular destruction and its opposite is life; therefore, life is generation, and to live is to be generated. This is absurd. Therefore, life and soul are not identical.' Here the premise 'Life and soul are identical' is tacitly assumed, and its contradictory stated as the conclusion of the *argumentum ad absurdum.*

Under this head, Aristotle includes not only cases in which the *argumentum ad absurdum* is employed, but all cases in which a conclusion is drawn from a premise which does not

The irrelevant thesis when used as a premise involves four terms.

necessitate it. In the *Analytica Priora*, he says: "The most "obvious case of the irrelevance of the thesis to the con- "clusion is when the thesis is not connected by any middle "term with the conclusion" (ii., 19). It thus appears that this fallacy involves, at any rate in some instances, the formal syllogistic fallacy of four terms. But when it is considered generally, its essential connexion with proof by *reductio ad impossibile* classes it more appropriately as a fallacy incident to method.

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